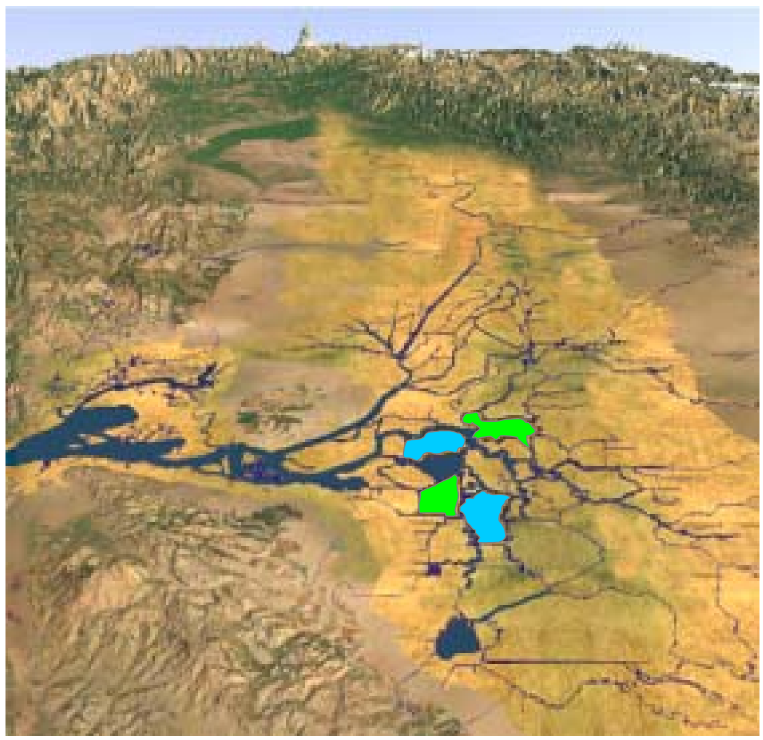
INTEGRATED STORAGE INVESTIGATIONS

IN-DELTA STORAGE PROGRAM
STATE FEASIBILITY STUDY
CALFED SCIENCE PANEL REVIEW

INFORMATION PACKAGE
CALFED SCIENCE PUBLIC WORKSHOP



CALIFORNIA BAY-DELTA AUTHORITY

DEPARTMENT OF WATER RESOURCES STATE OF CALIFORNIA

Division of Planning and Local Assistance
Department of Water Resources

August 20, 2003

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GENERAL INFORMATION

1. Introduction

The CALFED Record of Decision (ROD) includes a general commitment for independent science panel review in all applicable decisions. This general commitment is made on Page 75 of the August 28, 2000 ROD and states that specific independent science panels may be convened for all components of the program as standing bodies or on as needed basis. The Science Program will not be directly involved in making regulatory decisions, but rather in ensuring that CALFED, and the CALFED agencies, are incorporating the best available knowledge into activities and decisions that are made, as well as continuously working towards narrowing scientific uncertainties, bettering knowledge, and advancing the debate. The CALFED Science Program reviews can serve as a science clearinghouse for the CALFED agencies and identify and articulate areas of scientific uncertainty relevant to key issues. An overarching principle of the Science Program is adaptive management and new information and scientific interpretations will be developed through adaptive management, as the programs progress, and will be used to confirm or modify problem definitions, conceptual models, research, and implementation actions.

In 2001, the California Department of Water Resources (DWR) and the CALFED Bay-Delta Program, with technical assistance from the U.S. Bureau of Reclamation (Reclamation), conducted a joint planning study to evaluate the Delta Wetlands Project and other In-Delta storage options for contributing to CALFED water supply reliability and ecosystem restoration objectives. The main purpose of these investigations was to determine if the proposed DW project was technically and financially feasible. The joint planning study, which was completed in May 2002, concluded that for public ownership requires modifications and additional analyses.

In August 2002, the CALFED Science Panel for the In-Delta Storage Program finished its initial evaluation of water quality, environmental, and operations studies completed in May 2002. The Science Panel Summary Review Report is included as Section B and individual Panel member reviews are included in Section D of the Information Package. The August 2002 Review mainly highlighted the need for water quality and operational studies and there were serious concerns in the environmental evaluations area.

2. August 2003 Summary Review Follow Up

CALFED Science, DWR Surface Storage Branch and Reclamation staff held several discussions to find ways to include a mechanism or opportunity for interaction during the review process and help improve the exchange of information to carry out the short- term and long-term studies. A meeting with the Science Panel members was held on March 27, 2003 on which short-term and long-term work understanding was created. The short-term study tasks for In-Delta Storage Program focus on reducing uncertainty and risk using the current experimental and modeling work and making further improvements to available models. The purpose is to help participating agencies and management make an informed decision. The short-term work required for the

State Feasibility Study will also fill requirements of the In-Delta Storage Program Work Plan for FY 2003 approved by BDPAC in June 2002. The water quality short-term studies included:

a. Field and laboratory peat soil and biomass interaction experiments;
b. CALSIM II operations modeling studies;
c. DSM2 Model studies for DOC based on logic developed through experiments and CALSIM operations;
d. DSM2 temperature and DO studies; and
e. Reservoir stratification studies to determine if stratification is a problem.

Preliminary findings of the Flow Science Inc. application of the Dynamic Reservoir Simulation Model (DYRESM) for stratification were sent to the Panel and their review comments are included in Section C.

3. Purpose of CALFD Science Workshop

The August public workshop provides an opportunity to constructively address important project related issues and discuss the ongoing process of reducing uncertainty with specific short-term and long-term scientific investigations that will assist decision-makers for the In-Delta Storage Program. Results from studies completed since the Panel reviewed the May 2002 Planning Reports are presented in the July 2003 Feasibility reports and will be discussed at the August workshop. Comments and recommendations from the review are discussed in the Information Package in Section B to facilitate the best use of time at the August 2003 public workshop. It is intended that the August workshop will facilitate interactive discussion among panel members, program staff and stakeholders and result in the identification of areas of consensus among participants.

The CALFED Science Panel will entertain presentations from the study teams, hold sessions for discussions and will make recommendations on further action.

4. Project Description

The In-Delta Storage Project includes the same islands as the DW Project - storage on Webb Tract and Bacon Island with Holland Tract and Bouldin Island as habitat islands for impact mitigation. However, the re-engineered Project design differs from the DW project by incorporating into the design:

- new embankment design and four consolidated inlet and outlet structures
- new project operations
- resolving local water quality issues through field experimentation and modeling
- revised Habitat Management Plans
- detailed risk and economic analysis

The State Feasibility Study objective is to provide technical and financial information to the CALFED agencies that will decide if the project can be implemented with an acceptable level of risk and the project would provide water supply reliability and ecosystem restoration benefits at a reasonable cost. The criteria to be used for State feasibility level determination were established during discussions of the DWR Independent Board of Consultants Meeting in May 2003. The general guideline included:

- no major changes or surprises in the project design and costs as the project moves into final design, construction, and operation; and
- no possibility of fatal flaws in the project that would jeopardize the implementation of the project.

AGENDA

CALFED SCIENCE PUBLIC WORKSHOP
August 20, 2003

Place: Auditorium, Building No. 8
714 P Street, Sacramento, CA 95814

Time: 8:00 am to 4:00 pm

8:00 am – 8:10 am	Introductions	(Sam Luoma, Chief Scientist, CALFED Science)
8:10 am – 8:20 am	Project Background and Importance of Review	(Steve Roberts)
8:20 am – 8:30 am	Operations Concept and Study Approach	(Pal Sandhu)
8:30 am – 9:00 am	Field Investigations	(Robert Duvall)
9:00 am – 9:45 am	Operations Modeling	(Dan Easton)
9:45 am – 10:00 am	Break	
10:00 am – 10:45 am	DSM2 Modeling	(Mike Mierzwa)
10:45 am – 11:15 am	DO and Temperature Studies	(Hari Rajbhandari)
11:15 am – 11:45 am	Reservoir Stratification Studies	(Susan Paulson)
11:45 am – 12:00 am	Questions	
12:00 noon - 1:00 pm	Lunch Break	
1:00 pm – 2:00 pm	Science Panel Discussions	(Closed Session)
2:00 pm – 3:30 pm	Public Session Discussions	(All Participants)
3:30 pm – 4:00 pm	Closing Remarks	(Science Panel)

CHARGE TO THE CALFED SCIENCE PANEL

- Determine if the State Feasibility Study adequately evaluated the range of environmental, operational and water quality issues necessary to reasonably estimate the operational performance of the project. Identify the specific issues that need more evaluation during the EIR/EIS Process.

- Evaluate if the State Feasibility Study used adequate scientific approaches and documentation to address the identified issues. Make specific recommendations for short-term and long-term actions.

- Provide interactive guidance on what research and studies are needed to bring together planning and academic perspectives in an ecosystem-based adaptive management approach for the In-Delta Storage Program in relationship to other CALFED programs.

DOCUMENTS PROVIDED UNDER SEPARATE COVER

1. In-Delta Storage Program State Feasibility Study Draft Executive Summary, Division of Planning and Local Assistance, California Department of Water Resources, July 2003.

2. In-Delta Storage Program State Feasibility Study Draft Report on Operations, Division of Planning and Local Assistance, California Department of water Resources, July 2003.

3. In-Delta Storage Program State Feasibility Study Draft Report on Water Quality Investigations, Division of Planning and Local Assistance, California Department of water Resources, July 2003.

4. In-Delta Storage Program State Feasibility Study Draft Report on Environmental Evaluations, Division of Planning and Local Assistance, California Department of water Resources, July 2003.

SECTION - A

In-Delta Storage Program
State Feasibility Study

Response to May 2002 Reports Science Summary Review

SECTION A: RESPONSE TO MAY 2002 REPORTS SCIENCE SUMMARY REVIEW

1. Introduction

Discussions of the In-Delta Storage Program staff (IDS staff) and the CALFED Science Panel (Panel) recognized the importance of water quality and environmental studies in determining the feasibility of the In-Delta Storage Project. The complex ecology of the reservoir islands and the Delta make it difficult to perform such studies. IDS staff and the Panel are in agreement that the presentation of multiple management options should be integrated with adaptive management strategies. Through this science review process, IDS staff has undertaken additional experimentation, mathematical simulations and literature reviews. IDS staff will continue to address remaining uncertainty with application of short-term and long-term techniques.

2. Issues Raised by the CALFED Science Review Panel

The Science Panel provided valuable comments regarding water quality and other issues. IDS staff and the Panel are in agreement that more studies are needed to further reduce uncertainty. Further work was recommended frequently in both the Panels' comments and the Program's planning and feasibility reports.

Many of the comments and recommendations in the reviews have been addressed in the July 2003 State Feasibility Study water quality and environmental evaluation reports. A follow up meeting of the IDS staff and the Panel members was held on March 27, 2003 to facilitate discussions.

In the following sections, responses are provided to the following issues and questions raised by the Panel.

- Concerns with using data obtained from physical models or mesocosms to estimate carbon loading in the reservoirs.

- Because the water quality studies were empirical, the applicability of the results to the proposed reservoir islands was questioned.

- DSM2 and CALSIM Modeling applications indicated large uncertainties and differences between the observed and calculated concentrations of the water quality constituents. Not enough information was available on models calibration and validation.

- Simplified assumptions made in the Dissolved oxygen and temperature modeling and information was insufficient to assess the real impact of discharged waters o water quality in the Delta.

- Recommendations regarding the development of a conceptual model were made by the Panel.

These issues contained numerous specific comments which are also addressed in the responses in the following sections...

3. Field Studies

Issue

- ***Concerns with using data obtained from physical models or mesocosms to estimate carbon loading in the reservoirs.***

Response

The mesocosm experiments and modeling approach to estimating carbon loading in the proposed reservoir was to obtain a feasibility study level estimate within the budget and schedule of the In Delta Storage Program. Past and current studies are part of the project development schedule designed to incrementally analyze the proposal, to first assess the projects' feasibility and then perform more specific studies to obtain the data necessary to obtain water quality and environmental permits, and to comply with NEPA/CEQA.

The mesocosm approach began with the SMARTS studies and has been supplemented by additional work since the last Science Panel review. The new work was done in 2002 and 2003 on organic carbon loading from peat soil and included work on biological productivity. TOC, chlorophyll and mercury issues were also addressed.

A review of the SMARTS studies by Dr. K. R. Reddy found problems in the studies but also recognized the usefulness of the data, breadth of data that was collected, and potential for greater understanding with additional analysis of the data. The new replicated and controlled mesocosm studies use peat soil from Bacon Island. The mesocosms are simple models that represent reservoir operations by simulating diversion and discharges and include biological processes. Methods and results from the 2002 study as well as some preliminary 2003 results are presented in Chapter 3 of the Water Quality Investigations July 2003 Report.

The Summary Review recommended that measurements of carbon fluxes from peat soils should be done using either intact soil cores or *in-situ* mesocosms. Soils disturbance in the current mesocosm studies are thought to be representative of the soils on these agricultural islands as these soils will also be in a disturbed state when flooded. Further, Dr. K. R. Reddy reviewed the 1998 and 1999 SMARTS experiments and concluded that peat depths should not affect carbon release to the water column because release is primarily a function of concentration gradients established across only the few centimeters of the soil/water interface. The agricultural islands that will be flooded are frequently tilled to a depth of 46 to 51 centimeters (Artemio Tapia personal communication 2002). The level of disturbance from tillage goes well beyond the top few centimeters of soil that will form the soil/water interface. Reservoir construction activities will also cause soil disturbance and when the islands are initially flooded, water movement flowing over the soil, wave action and gas bubbles escaping from the soil will cause further disturbance of the soil.

The Summary Review suggested that Twitchell and Mildred islands could be considered analogs for the reservoir islands. DWR compared results from the mesocosm studies with field data from larger scale peat soil systems like the Twitchell Island wetlands. Organic carbon loading rates derived from the mesocosm studies are consistent with limited data from these Twitchell Island wetlands. Selection of experimental islands for future work is an important issue that should be discussed at the workshop.

Wide ranging estimates provided by experts for carbon loading rates and concentrations in the reservoir islands, the level of disagreement among experts and the lack of data and studies sites that are analogous or reasonably representative of flooded reservoir islands are major reasons for the mesocosm approach. These studies were designed and conducted to address specific project needs and timelines.

Issue

- ***Because the water quality studies were empirical the applicability of the results to the proposed reservoir islands is questionable.***

Response

Empirical studies are thought to be appropriate for the In-Delta Program because direct collection of data or information about the system is impossible because the reservoir islands (the systems) don't yet exist. Additionally, subsided peat soil islands flooded to a depth four feet above sea level and operated under a variable, anthropogenic hydrology in an already highly modified system (the Delta) are unique; analogous systems may not exist. The mesocosm studies were done to better understand and to provide representative data for the unconstructed reservoirs. While these studies are limited, they are perhaps the best we have and could be more analogous to the proposed reservoir islands than are a shallow, flow through wetland on Twitchell Island or a tidal Mildred Island.

Task 6 of the Summary Review recommends monitoring of biogeochemical processes on Mildred and Twitchell Islands. However, it is not clear from the seven individual reviews or the Summary how applicable or analogous these islands are to the proposed reservoir islands or how Project issues can be addressed by this monitoring. The carbon loading of the reservoir islands is being estimated by integrating data and information from physical models of the reservoirs (mesocosms) with conceptual and mathematical models. Guidance from the Panel would be appreciated regarding how information from monitoring Mildred or Twitchell Islands or other sites could be used in the analyses of proposed reservoir islands and addressing specific project needs.

The Panel suggested *in situ* mesocosms but the meaning of *in situ* mesocosms is not clear. If the intent was to suggest constructing mesocosms on Webb Tract or Bacon Island this is a logical next step and has been considered by DWR Staff. However, this idea may not be easy to implement considering the current short timeline of the project. Engineering designs, permission from land owners and CEQA and permits may be required before starting a large research project like this. In any case, it would be good to discuss possible similarities and differences between "on site" mesocosms and those at Bryte which used on site soil, river water, and flora and fauna from the Delta.

One thing missing from the mesocosms at Bryte is groundwater inflow, which has been identified as the most important aspect of loads on Twitchell (Fujii personal communication 2003). Whether or not the lack of simulated groundwater inflow in the Bryte mesocosms is a problem for the reservoir islands should be discussed in the meeting. Recent modeling studies indicate organic carbon loading from seepage return flows is not a significant problem.

4. Mathematical Model Applications to In-Delta Storage Project

Issue

- ***DSM2 and CALSIM modeling applications indicated large uncertainties and differences between the observed and calculated concentrations of the water quality constituents. Not enough information was available on models calibration and validation.***

Response

A CALSIM daily model has been developed for the Delta and is being used for reservoir operation studies. The DSM2 daily model is also being applied and uses hydrologic flow input from CALSIM to check if water quality constraints from the SWRCB permit are being met. A second CALSIM iteration, reservoir reoperation is then run to meet water quality standards. Additional information on the ongoing development and application of CALSIM and DSM2 is provided below. The models assumed mixed flow conditions for reservoir modeling. The issue of stratification in the reservoir islands was raised by the CALFED Science Review Panel in the Summary review. Later on discussions at the March 27, 2003 meeting, Panel members suggested that reservoir stratification studies be conducted. In response, Flow Science Inc. performed DYRESM model stratification studies which are presented in Appendix C of the Draft Report on Water Quality Investigations, July 2003.

4.1 DSM2 Application

DSM2 is a state of the art public domain 1-D model capable of simulating hydrodynamics, water quality and particle tracking. DSM2 has been the primary tool used by DWR Delta Modeling Section to study the Sacramento San Joaquin River Delta since the summer of 1997. The best sources of information about DSM2 are:

1- Delta Modeling Section website

   http://modeling.water.ca.gov/delta/index.html

2- Delta Modeling Section's Annual reports

(These can be downloaded from the web-site)

4.1.1 Calibration/Validation

DSM2 was originally calibrated/validated in 1997. In 2000, DSM2 was recalibrated extensively in a multi-agency effort under the auspices of the Interagency Ecological Program. The main focus of this effort was on stage, flow and EC data throughout the Delta. For more detailed information about this effort, refer to Chapter 2, 2001 Delta Modeling Section annual report at:

http://modeling.water.ca.gov/delta/reports/annrpt/2001/

Graphical plots for DSM2 validation are available via a clickable map at:

http://modeling.water.ca.gov/delta/studies/validation2000/map.html

Considering the complex nature of the Delta, the comparison of model results with the field data appears quite reasonable.

4.1.2 DOC Validation

New DSM2 modeling studies for DOC have been conducted and results are presented in Chapter 2 of the Draft Report on Water Quality Investigations, July 2003.

In 2001, a separate study was conducted to test the capability of DSM2 in simulating the transport of disinfection by-product (DBP) precursor surrogates in the Delta. The study focused on dissolved organic carbon (DOC) and ultraviolet absorbency at 254 nm (UVA) – two widely accepted DBP precursor surrogates. The main difficulty was the lack of the complete regularly scheduled data set for the boundary conditions needed to run the model. Based on a limited data set containing a few monthly grab-sample measurements, a set of representative monthly averaged values was generated. To make the analysis simple, both DOC and UVA were simulated as conservative constituents. It was felt that since elevated values of DOC and UVA usually occur in winter, when flows are high and travel times are usually short enough, this assumption was a good approximation. Since the data at the interior Delta locations were also based on few monthly grab sample data, model comparison was shown based on monthly maximum, minimum, and average. Considering the level of approximations used in this analysis, the comparison of model output looks reasonable at almost all the locations. The model seems to capture the peak events in terms of timing and magnitude. The one noticeable exception was North Bay Aqueduct. This was not surprising since the magnitude of net flows in the North Bay Aqueduct is fairly small. A significant portion of the water exported during winter is the island runoff with elevated concentration of DOC and UVA. In DSM2, the amount of water exchange between the rivers and the agricultural lands are estimated using DICU (Delta Island Consumptive Use) which is basically a water demand model. These values are very crude approximations. As such, it is not a surprise that the DOC and UVA estimates at North Bay

Aqueduct are not matching well with the field data. For more information, on DOC and UVA validation, please refer to Chapter 3, 2001 Delta Modeling Section annual report at:

http://modeling.water.ca.gov/delta/reports/annrpt/2001/

4.2 Dissolved Oxygen (DO) and Temperature Modeling

Issue

- ***Simplified assumptions made in the Dissolved oxygen and temperature modeling and information was insufficient to assess the real impact of discharged waters on water quality in the Delta.***

Response

May 2002 DO and Temperature studies were based on Reclamation's Spreadsheet Model and assumptions were made due to lack of available data. New DSM2 modeling studies for DO and Temperature have been conducted and results are presented in Chapter 4 of the Draft Report on Water Quality Investigations, July 2003.

DSM2 is capable of simulating the dynamics of primary production including dissolved oxygen, phytoplankton, nutrients, and temperature. A single water quality variable or any combination of 11 water quality variables can be modeled as specified by the user. Changes in mass of constituents because of decay, growth, and biochemical transformations are simulated using interconstituent relationships derived from the literature.

Simulation of dissolved oxygen requires information on water temperature, BOD, chlorophyll, organic nitrogen, ammonia nitrogen, nitrite nitrogen, nitrate nitrogen, organic phosphorus, dissolved phosphorus (ortho-phosphate), and EC in the Delta. Continuous sources of data were available for DO, temperature, and EC at hourly intervals only for some selected stations near model boundaries. The data for other constituents is usually approximated based on any grab sample data that may be available. For more information on the mathematical formulation used for the interconstituent relationship used in DSM2, please refer to Chapter 3, 1998 Delta Modeling Section annual report at:

http://modeling.water.ca.gov/delta/reports/annrpt/1998/

The process of calibration for DO is data intensive. The first calibration effort primarily focused in the San Joaquin River extending to the Stockton Ship Channel near Turner/Columbia Cut, where low DO levels have been a major concern. Based primarily on availability of data, the period of August through October of 1998 was chosen for calibration. The process of calibration began with the calibration of water temperature. Evaporation coefficients were adjusted until there was reasonable agreement between simulated and measured temperature as discussed below. During DO calibration, the following parameters were adjusted: algae (growth, respiration, settling, and mortality rates), nitrogen (organic nitrogen decay and oxidation rates of ammonia and nitrite), and sediment oxygen demand. Calibrated coefficients are within the range suggested in the literature.

Model validation for DO and temperature was done for the period from July through September 1999, when the flow in the San Joaquin River at Vernalis was considerably lower than the San Joaquin River flows from the calibration period. The rate coefficients adopted during calibration were kept the same during this simulation.

For more information on this activity please refer to Chapter 6, 2001 Delta Modeling Section annual report at:

http://modeling.water.ca.gov/delta/reports/annrpt/2001/

The Delta Modeling Section expanded the validation period to cover from July 1996, to December 2000. The focus area expanded to include areas in South Delta where some data was collected in 2000. This effort was part of an investigation to check the feasibility of adding auxiliary pumps over Grant Line Canal barrier, and thus increasing the flow in the Stockton Ship Channel. Please refer to "DSM2 Studies to investigate the Use of Auxiliary Flow Pumps across South Delta Structures" at:

http://modeling.water.ca.gov/branch/reports.html

4.2.1 Recent Temperature and DO Work in DSM2

New DSM2 modeling studies for DO and Temperature have been conducted and results are presented in Chapter 4 of the Draft Report on Water Quality Investigations, July 2003.

The first set of DO and temperature simulations are be based on a CALSIM II reservoir operation scenario used to meet DOC criteria specified in the State Water Resources control Board (SWRCB) Decision 1643. Sixteen years of boundary condition data for temperature and DO are being approximated based on the closest hydrologic or the climatic conditions. For the temperature and DO of the In-Delta storage water release, we expect to use the output provided by Flow Science Inc. based on their reservoir modeling. Flow Science Inc. is conducting a study to determine if the reservoir islands will stratify and to predict temperature, DO and DOC differentials between the islands and adjacent channels for differential operational scenarios of the In-Delta Storage Project. The equations describing DO kinetics in DSM2 are documented in "The *Methodology for Flow and Salinity Estimates in the Sacramento-San Joaquin Delta and Suisun Marsh. 19th Annual Progress Report to the State Water Resources Control Board*", also available at the Delta Modeling web site http://modeling.water.ca.gov/delta/reports/annrpt/1998/chpt3.html. The reaction rate coefficients were calibrated and validated primarily for the Delta channels near Stockton and the South Delta.

DSM2 simulations of historical temperature indicate that the channel temperatures adjacent to the islands tend to be in the higher range in certain months, and hence the requirement of keeping the change below 1 degree Fahrenheit will most likely be governing. The criteria do not state if the standards (for both temperature and DO) are to be met on the basis of instantaneous value or net tidal day average (14 -day, 1-day etc.) It is highly likely that if the temperature differentials were to be satisfied on an instantaneous basis, temperature criteria will be violated. This is because at the time when tide changes its direction, water quality measured in the channel near the discharge outlet would primarily reflect that of the In-Delta storage. DSM2 simulations will primarily focus on meeting the criteria based on daily average of simulated results.

DSM2 Simulations of historical DO indicate that the channel oxygen levels adjacent to the islands are generally well above 6 mg/l. If the requirement of keeping the reservoir at or above 6 mg/l is met, it is very likely that the requirement of the discharged water not depressing the adjacent channel DO levels to the levels of 5 mg/l or below would also be met.

It should be of interest to the CALFED Science Panel that the Regional Water Quality Control Board is performing an independent validation of DSM2 for dissolved oxygen simulation in the Stockton Deep Water Ship Channel. The validation period is for 1999-2002. Additionally, Delta Modeling Section has been investigating multi-dimensional models, in order to develop a tool that can solve problems that cannot be easily solved using any current models including DSM2.

4.3 Suitability of 3-D Models to Simulate DOC, DO, and Temperature

As it was described above, past DOC, DO, and temperature simulations had to rely on numerous assumptions on the boundary conditions used. Most of these assumptions relied on the best engineering judgment however the magnitude of the errors in the assumptions at times may have been significant. The modeling study results whether 1-D, 2-D or 3-D model are based on the data used. However, a 3-D model may be suitable in the following cases:

1- The problem being solved is an extremely well defined problem, one where all the input parameters have been accurately measured.
2- The physical processes are complex enough that the results from a 1-D or a 2-D model would lead to erroneous conclusions.

For most areas of the Delta, there are not enough complexities to warrant the use of 2 or 3-D models. The Delta Modeling Section has illustrated the suitability of DSM2 as ‘a model of choice’ for solving the problems in the Delta. It has been proven (especially for areas in the interior Delta where channels are well aligned), that a one-dimensional representation is adequate. This is especially apparent in the model validation plots described above (flow, stage, EC). These are clearly well defined problems since regularly scheduled field measurements are available for most boundary conditions (with the exception of the water exchange between channels and agricultural lands). Under such conditions, DSM2 seems to capture field conditions much more closely.

One possible exception may be the storage reservoirs. All the modeling done in the past, primarily focused on moving bodies of water. Processes affecting DO and temperature in a lake setting could be quite complicated. While it may be possible to try and calibrate DSM2 to predict DO and temperature in a reservoir setting with a reasonable accuracy, this needs to be tested.

4.4 Comments Addressed to Specific Areas of the Review Document

4.4.1 Time scales, spatial scales, and time frames of study components

Daily DSM2 runs were done for In-Delta operations, but results in the report tables were shown in monthly terms for simplicity. The same is true for CALSIM runs. The CALSIM Model used for the Delta portion is a daily time step model. However, results were included as monthly in the report.

Some qualitative estimates of DO and temperatures were discussed in the reports cited above describing calibration and validation. However, more quantitative estimates can be done in the next set of studies. DSM2 results of DO and temperature are usually reported in hourly resolution (a probable time scale of expected monitoring), and can be provided in the same resolution, if necessary. The DSM2 hourly output should provide useful information on DO and temperature diurnal variation, although it is likely that some accuracy will be compromised due to the CALSIM daily time steps.

4.4.2 No simulation for drying of the reservoir beds

DSM2 cannot simulate complete drying of reservoir beds. For this study 0.5 ft was used as a minimum water depth modeled to avoid drying of reservoir at times of low flows. The effects of this restriction on DOC simulation were not considered a concern, since the relationship for DOC concentrations, (as a function of water depth) as coded in DSM2 for this study, would not be valid at very low water depths. Moreover, In-Delta reservoirs will be operated on a year-round basis in coordination with upstream reservoirs. With management of diversion and release operations, reservoirs would not reach extreme low or dry bed stage. Also, with water circulation operations, DOC concentrations could be controlled.

4.4.3 Need for common scenarios and assumptions

Efforts will be made to base the study on the same operational, geometric, and hydrologic scenarios as far as it is practical to do so. Currently Year 2020 level is the latest level of demand that CALSIM operation scenario is available for. Year 2030 level of demand is being developed and should be available for use soon.

4.4.4 Ecosystem Functions and Process Integration

Constituent dynamics are integrated in DSM2 as far as DO, temperature and selected nutrients are concerned. In the interest of time, process integration most likely will have to be limited to what is available in DSM2 currently.

4.4.5 DOC in Seepage Return Flows

Seepage return flows have been included in the DSM2 Model. All recent In-Delta Storage Project studies include DOC loading in seepage return flows due to seepage water flowing through peat soils and then being picked by seepage pumps and returned to the resrvoir. Details on seepage treatment in DSM2 are presented in Chapter 2 of the draft Report on Water Quality Investigations, July 2003.

4.5 CALSIM Model Application

In the technical review of the In-Delta Storage Program studies, the panel had three requests pertaining to CALSIM - documentation of the water distribution mechanism, assessment of CVPIA b(2) and EWA impacts, and quantification of uncertainties. To date, the most thorough review of CALSIM input and functionality is found in the appendices of the Benchmark Studies Assumptions (September 2002). The document can be downloaded from the following address:

http://modeling.water.ca.gov/hydro/studies/SWPReliability/index2.html

CALSIM II operations in the Delta for reservoir operations were done using a daily time step. CALSIM II with CVPIA b(2) and EWA operations had not been released for use in studies until very recently. The present version only performs simulations at a monthly time-step which is too large of a time-scale to accurately gage island reservoir impacts. As such, work is under way to implement the b(2) and EWA operations at a daily time-step. In the IN-Delta modeling studies, there are no diversions to island reservoirs during April and May and this would be similar to including VAMP actions under CVPIA b(2). In fact it eliminates uncertainty of further reductions in yield. EWA operations have been incorporated.

Uncertainty in the CALSIM In-Delta Storage Program study results is very hard to quantify. Development of a water resource system planning model requires informational assumptions to account for the unknown as well as simplifying assumptions to reasonably represent complex processes within the limits of the applied modeling technology. Because we are in the planning mode, most of these assumptions pertain to the future—future population, future land use, future Delta standards, etc. The recorded historical hydrology modified to reflect these future changes is used in a typical operational simulation. Some of these uncertainties are discussed in greater detail in The State Water Project Delivery Reliability Report available at http://swpdelivery.water.ca.gov. Sensitivity analyses of significant assumptions as well as recent years simulations will be completed to address some of these uncertainties.

4.5.1 CALSIM Updates

Since the December 2001 In-Delta Storage Program Feasibility Study Draft Report, the CALSIM Daily Operations Model (DOM) has been greatly enhanced. For the operational planning studies presented in the draft report, only Delta operations were performed at a daily time-step. All North of Delta (NOD) reservoir operations were simulated at a monthly time-step by CALSIM II – the SWP/CVP joint planning model. This created an unnatural disconnection between NOD and Delta operations. As modeled, all monthly changes in exports due to daily hydrologic variation had to be absorbed entirely by San Luis Reservoir. CALSIM II could react in the next month by releasing more or less water, but it still had no control over how the exports would then play out in the DOM.

To fix this, NOD operations are now included in the DOM. Rather than variably disaggregating Delta inflow and solely looking at the impact on Delta operations, the upstream uncontrolled accretions are now disaggregated and the NOD reservoirs are allowed to absorb this variation where it is both possible and beneficial. In effect, the entire system now reacts to daily variability and there is no disconnection between NOD reservoir operations and South of Delta exports. Documentation of the DOM should be complete by the end of June.

4.5.2 Recent CALSIM II Model Operation Studies

New CALSIM II modeling studies for D1643 and WQMP including DOC new logic have been conducted and results are presented in the Draft Report on Operations, July 2003.

5. Conceptual Model Development for In-Delta Reservoir Islands

The Summary review recommended the development of a comprehensive, process-level, mechanistic-based conceptual model of the carbon dynamics in the reservoir system, specifically including release of DOC from peat soils, biological productivity, and the carbon dynamics and cycling processes associated with these carbon sources. Conceptual models will be developed for a variety of environmental resource categories to facilitate analyses in meeting the NEPA/CEQA requirements. A discussion of conceptual model development for the reservoir islands is provided in Chapter 3 of the Draft Report on Water Quality Investigations, July 2003.

SECTION – B

In-Delta Storage Program
State Feasibility Study

List of CALFED Science Panel Members

SECTION B: LIST OF CALFED SCIENCE PANEL MEMBERS

Note: Order of Panel members and anonymous reviews are not related

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SECTION – C

In Delta Storage Program
State Feasibility Study

Summary Review of May 2002 Reports

SECTION C: SUMMARY REVIEW OF MAY 2002 REPORTS

Summary of Scientific and Technical Review Panel Comments on Water Quality and Environmental Aspects of the In-Delta Storage Program's Reports

1. Introduction

The goal of the In-Delta Storage Program is to increase water supply reliability, improve operational flexibility, and allow water to be conserved during wet periods. The purpose of the current evaluation contained in the reports is to determine the technical feasibility of the Delta Wetlands Project or other in-Delta storage options. Additional reports examine the financial feasibility of the project, and a separate review panel has examined engineering aspects of the project. This panel has focused on the water quality and environmental implications of the projects and has considered engineering and operational aspects only as necessary to complete their assessment.

Our summary of the review panel's comments is structured as follows:

- Comprehensiveness - have the studies adequately considered the range of issues necessary to fully assess the water quality and environmental implications of this project, and if not what are the key factors that need to be addressed?
- Scientific Validity – do the studies use adequate approaches (experimental, empirical, and numerical) to address the issues they identify, are these approaches adequately documented, especially regarding their assumptions and uncertainties, and how could the studies be improved?
- Future Work – what further research and studies are needed to fill in critical gaps or reduce uncertainties, what monitoring or evaluation is needed if the project is implemented, and what immediate steps should be taken to move the project forward?

2. Comprehensiveness

2.1 Review

Overall, the scientific reviewers generally believed DWR did a commendable job of working with the tools, data, and time available. Reviewers expressed that many of the individual assessments and approaches have merit and provide a basis for further investigation. However, the studies are highly empirical and derived from incomplete information about the system, rendering forecasts of likely impacts of the DWP limited in their generality and validity. A proper evaluation of the proposal will require new and different data, additional and expanded, more mechanistic and integrated models, and more rigorous analysis of uncertainties.

The evaluation of water quality and environmental issues in these studies is seemingly driven by two needs:

1) operational criteria defined by the State Water Resources Control Board in their Decision 1643, and

2) assessments necessary to mitigate for impacts of water storage operations on state-listed threatened species, jurisdictional wetlands, and winter waterfowl habitat.

As such, the scope of the studies is limited to those issues, which are raised, in a regulatory context, rather than in the context of understanding the implications of the water storage operations in the broader hydrologic and ecological context. As a consequence water quality studies focus on dissolved organic carbon (DOC) (TOC needs to be considered), chloride, disinfection byproduct (DBP) precursors (only total trihalomethanes, TTHMs, were considered), dissolved oxygen (DO), electrical conductivity (EC), chloride, and temperature.

Reviewers called for a broadening of this approach in two ways:

- A more ecosystem-based assessment of the in-delta storage plans, and their relationship to ERP goals for the Delta.
- A more explicit programmatic assessment of how these storage projects interface with other aspects of SWP/CVP operations including EWA and CVPIA b(2) waters.

Reviewers recognized that the Delta is a critical element of the state's water transfer system and that the addition of water quality, biological and ecological considerations on top of conveyance systems greatly complicates the assessment of the water storage projects. However, consideration of ecosystem functions is considered essential to forecasting the changes that will be associated with in-delta storage. These include ecological and biogeochemical processes, such as elemental inputs, recycling and losses, primary production and decomposition, fate and transfer of pollutants, and food web interactions.

The existing list of chemical features misses important system linkages. For example, the biological productivity studies are lacking essential components such as quantitative treatment of chlorophyll a (phytoplankton biomass being an important dependent variable for assessing ecosystem response), suspended particulate matter (as primary production may be light limited this will be an important control on chlorophyll a as well as an influence on potential sedimentation on the reservoirs), and biological oxygen demand (which may be a more important factor to assess in operating for water quality criteria than the DO content of the discharged waters). Understanding these water storage projects as part of the Delta ecosystem is essential to understanding the implications for ecosystem restoration goals as well as water conveyance.

Related to this lack of a systemic approach to project assessment is the lack of detailed attention to the potential mercury and methyl mercury problems in the proposed project. Methyl mercury production is enhanced by an adequate supply of organically-bound mercury, very warm water temperatures, anaerobic conditions, high organic matter contents and dark water which can block UV demethylation – all conditions likely to occur within the proposed reservoirs. Although SWRCB Decision 1643 does not address mercury specifically, Delta Waterways are on the State's 303 (d) list as impaired for mercury and thus the implications of delta storage operations for mercury must be addressed.

In addition to the lack of detailed consideration of the implications of the project for the Delta ecosystem, reviewers also noted the very brief attention paid to long-term changes in hydrologic drivers of the ecosystem – most notably those associated with climate change. The state of California and the Delta are likely highly sensitive to subtle shifts in temperature and weather patterns associated with global climate change scenarios. Water storage and conveyance concerns will change as snow pack and snow melt patterns change in the Sierras. Explicit consideration of future changes in hydrologic drivers must be linked with the ecosystem evaluation of delta storage options.

Long-term management of the water in the Delta will likely include operational as well as engineering changes of the kind proposed here. A variety of operational changes are converging toward decision points in the near future, and this project must be considered in the future operational context. Definitions of environmental water and environmental management are rapidly changing, as perhaps they should in an adaptive management context. Among the programmatic/operational issues that may affect the role of in-delta storage are the renewal of long-term water contracts; modifications to cross channel management based on new science; questions regarding the status of CVPIA b(2) water; the role of EWA water vis a vis b(2) unmet needs; and proposals to increase pumping rates. While a decision on moving forward with in-delta storage plans may not be able to wait until all of these issues are resolved (and as new ones will likely arise) the project must be viewed in a more varied context for future water operation

2.2 Recommendations - Comprehensiveness

The reports and assessments are responsive to the statutory climate within which implementation needs to proceed. A more holistic approach is necessary for CALFED to evaluate the implications of in-delta water storage for its goal of restoring ecosystem health, as well as water quality and water supply reliability goals.

The first step in this broader approach to considering in-delta storage is the development of the conceptual model showing the processes, and their linkages, both driving project operation and affected by project operation. Specifically this conceptualization should embrace:

- the project in the context of the water conveyance system and its hydrologic and programmatic controls,
- the project in the context of the Delta ecosystem and the spatial and temporal patterns of ecosystem functions, and
- the detailed hydrologic and ecological dynamics of the reservoirs and surrounding channels, including the operation of intakes/discharges.

Because of the complexity of the system within which the project is set, a series of nested conceptual models is recommended: the water conveyance system (largely hydrologic, considering EC and operations), the delta (including ecosystem and water quality considerations), and the reservoirs/channels (including water quality and ecosystem processes).

The models will demonstrate the relative importance of the project for the various scales of the hydrologic and ecological system. It is not necessary to quantify the relationships among all model components. Rather, the models should be used to identify process linkages to which project operation is sensitive, and process linkages which are sensitive to project operation. In addition, the level of scientific certainty or uncertainty regarding the linkages also should be shown in the models, allowing prioritization of research and data collection needs.

Such conceptual models can also be used to evaluate the project in the light of current and proposed, or even hypothesized, water transfer operations to determine its potential role in the future of water supply and ecosystem restoration.

3. Scientific Validity

3.1 DOC and Other Drinking Water Concerns

One of the primary concerns related to the technical feasibility of the proposed in-delta storage project is how flooding peat islands will affect the quality of the water released to the Delta channels and potentially diverted for drinking water. Current and planned regulation of DOC are challenging the drinking water utilities and CALFED, DWR, and other state, local, and federal agencies to find innovative and robust means to comply with these regulatory and human health constraints. At times water diverted from the Delta can exceed the U.S. Environmental Protection Agency's current maximum contaminant level for disinfection byproducts (e.g., trihalomethanes, THMs) when chlorinated for drinking. Certain forms of DOC, as well as bromide, react with disinfectants, such as chlorine, to form carcinogenic and mutagenic byproducts (e.g., THMs). Therefore, it is extremely important that any changes to the Delta water-supply system (e.g., addition of reservoir island water storage and supply) not further degrade drinking water quality, especially with respect to increasing DOC and DBP precursor levels in the channel water. Because of the importance of the DOC water quality issue, this summary review section treats this topic separately.

The reviewers recognized and appreciated all of the effort put forth in developing estimates of DOC and THM precursors potentially contributed to the Delta by the proposed flooded island reservoir. However, the reviewers had several criticisms of the conceptual model, experimental approach and methods, and modeling of data. A general consensus of all reviewers was that the SMARTS experiments that estimated peat-derived DOC contributions did not use the state of the science to estimated DOC concentrations in reservoir water and failed to address the fundamental processes important to adequately understand, and therefore accurately model, release of DOC from flooded peat soils. This shortcoming calls into question the appropriateness of using these results to predict the concentrations of DOC and THM precursors that may result when the reservoir islands are flooded. In addition, the conceptual model for DOC release from peat soils is not complete, implying a lack of scientific understanding of the system and the underlying fundamental biogeochemical and hydrologic processes controlling the release DOC. Reviewers expressed concern over the high degree of uncertainty of almost all aspects of the DOC assessment, and the complete lack of error analysis further brings into question the credibility of the assessment. Thus, the validity and appropriateness of applying the results from the SMARTS studies to flooded islands is doubtful.

3.1.1 Conceptual Model

An appropriate conceptual model for release of DOC from flooded peat soils requires consideration of all significant biogeochemical and hydrologic processes affecting carbon cycling within the system. This process-level approach to understanding the system is important for identifying the key questions to answer or hypotheses to test, which in turn provides the guidance to design scientifically sound experiments that effectively address the questions or hypotheses. As voiced by several reviewers, the conceptual model for release of DOC from peat soils did not consider some of the most pertinent processes controlling the release of DOC from the soils.

The flux of DOC from the soil to the overlying water column depends on diffusive and convective transport of DOC across the soil-water interface. In the water column, mixing and hydrodynamic process will govern the distribution of DOC concentrations. Microbial activity in the peat soil is a potentially important DOC source term. However, microbial effects on DOC production may be relatively minor under flooded, reduced conditions. On the other hand, microbial decomposition of soil organic matter maybe very significant if the soils are exposed to oxygen through wetting and drying cycles, which may be unavoidable during drawdown of reservoir water levels to as low as 0.5 ft. The irregular topography of the island most likely means that some areas will be unsaturated and exposed to atmospheric oxygen. Studies on Twitchell Island have demonstrated the significant effect of wetting and drying cycles on the increased release of DOC from peat soils.

Another important consideration is the difference in the potential release of DOC from different soil layers. Upper, more oxidized peat soils tend to contribute much higher concentrations of DOC of different quality relative to deeper peat layers that have not been exposed to oxygen. This soil horizonation will be extremely important when considering the diffusive transport of DOC over time from the peat soil. The SMARTS tank experiments used only upper, oxidized peat soils that had potential to release much greater amounts of DOC when flooded compared to deeper peat soil zones, and omission of the lower, reduced peat layers may have affected the results obtained.

The importance of considering biological productivity was acknowledged but not included in the modeling of reservoir DOC concentrations. Algal and macrophyte growth and decay are well known sources of DOC and may be extremely important in determining DOC concentrations and quality in the water column. These biological processes may, in fact, dominate both DOC quantity and quality during critical times of the year. For instance, algal and macrophyte senescence and decomposition, and release of DOC may be greatest in late summer and early fall when river flows into the Delta are lowest and water releases from the reservoir islands may be critical. In addition, carbon quality data indicate that decomposition of some aquatic plants (e.g., algae and *lemna*) produces DOC with much higher propensities to form THMs, as much as 3 to 5 times more THMs form per mass of DOC compared to the peat-soil DOC. This example emphasized the necessity of quantitatively assessing DOC quality in the context of ecological, biological productivity, and carbon cycling processes in order to adequately evaluate and predict drinking water quality in the proposed reservoir islands. The current effort failed to take into account relevant processes such as these, making their assessment incomplete and inaccurate.

In general, many other processes (e.g., redox, hydrodynamic, nutrient cycling) and their effects on important ecosystem functions through controlling key ecosystem characteristics (e.g., DO and temperature dynamics) are not considered in their current conceptual model of the system. For example, consideration of nutrient supply and dynamics is essential because nutrient supply is directly related to plant growth, which, in turn, influences DOC levels and ecosystem function.

Nutrient loading could strongly affect the phytoplankton communities and benthic microbial communities that are resident in a reservoir system. For example, certain nuisance algae may proliferate under high nutrient conditions. This will alter the population dynamics of important phytoplankton, such as diatoms, as well as lead to changes in ambient environmental conditions, such as dissolved oxygen concentrations. Also, benthic microbial communities and invertebrates that process DOC and POC also may be affected by eutrophic conditions, affecting their population dynamics. These examples emphasize the need to consider processes such as nutrient cycling and its effects on ecosystem functions.

3.1.2 Experimental Approach and Methods

The reviewers had many concerns about the validity of the methods and use of soils in the SMARTS experiments. A question raised by most reviewers was why soils from the proposed reservoir islands were not used. The use of Twitchell Island soil may have been convenient, but there are no assurances (at least no data were presented) of the transferability of results from one soil to another. At the very least, analyses and experiments should have been conducted comparing organic matter and other soil characteristics of the two different soils. In addition, it is impossible to tell from the level of detail provided how sensitive the DOC values were to water depth, the nature of the soil, and the depth of the soil used in the experiment. A greater acknowledgement of the factors the experiment fails to encompass also was lacking. These shortcomings of the experimental design and execution emphasize the need for a well-defined conceptual model that incorporates the processes controlling the ‘release’ of DOC from peat soils and better identifies which processes were examined and those that were not.
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Although the methods involved in the manipulation of peat soils to fill the experimental tanks were not described in detail, the procedure most likely caused significant disturbance of the soil structure and integrity. Destruction of soil structure and integrity causes significant changes in the hydraulic properties of the soil, which, in turn, alters the soil’s transport properties. This experimental artifact undoubtedly altered the movement and release of DOC from the soil to the overlying water. Disturbance of the soil and increased exposure to atmospheric oxygen most likely caused additional oxidation of the soil organic matter, further perturbing the carbon dynamics and release of DOC relative to *in-situ* soil conditions. Altering the soils structure also will increase the amount of water-soil contact, most likely increasing the amount of DOC in interstitial soil water. Thus, the soil manipulations involved in the tank experiments call into question the validity of the data obtained.

3.1.3 Modeling of DOC

Reviewers were in agreement that the use of the logistics equations to model the release of DOC from the soil was a poor choice because this modeling technique does not account for any of the processes governing the release of DOC. Because of the lack of representation of the biogeochemical and hydrologic processes, applicability of the results is limited to the system from which the data were collected, making it questionable, at best, to transfer these results to the proposed reservoir islands. To be valid, predictive models must be built on a mechanistic understanding of the processes involved. In addition, several of the reviewers had serious, well-documented concerns about the accuracy of the model and the assumptions, development, and application of the logistics-equation approach used to model DOC release. An alternative approach for modeling water-column DOC is clearly needed that takes into account the shortcomings cited above.

Another important concern voiced by the reviewers was the seepage return estimates to reservoir-water DOC. The model used contained overly constrained boundary conditions, was not adequately evaluated or validated, and may not have been appropriate for describing the system. A two-dimensional model is not adequate to simulate a peat system, suggesting the need for a 3-D model.

Modeling of DOC using DSM2 showed large disparities between observed and calculated concentrations, at both high and low concentrations, indicating that the dynamics of the system are not being captured by the model in many cases. Model predictions improved when monthly averaged data are used, but this tends to obscure the concentration extremes, missing the temporal dynamics that may be the most relevant periods for the water utilities to respond to in terms of TOC and TTHM formation potentials.

The calculations used to derive UVA and TTHM are based on DOC, for which large uncertainties are associated. Thus, these calculated values also have large uncertainties associated with them. The modeling of channel-water DOC and UVA used 5^{th} order, nonlinear, polynomial regression equations to provide channel-water DOC and UVA inputs to the model. Not only is the 5^{th} order model inappropriate (a 2^{nd} order model probably is more valid), discrepancies between modeled and measured values were very high for many of the months and, in addition, the large uncertainties apparent in the modeled data set were not addressed. These examples further emphasize the need to assess uncertainty through error propagation analysis, and to better quantify uncertainty throughout the reports.

The above examples demonstrate the need for further assessment and refinement of the models, and quantification and incorporation of error and uncertainty.

3.2 Use of Modeling in Planning In-Delta Storage

While it is essential to use numerical models to assess the hydrodynamics and ecosystem processes occurring within reservoirs and in adjacent channels, it is also important to recognize that the level of detail required for making planning decisions may be substantially greater than that used in operation of the system. Several reviewers note the limitations of CALSIM and DSM2 in assessing the proposed project. In large part this may be because these models were designed to inform operational aspects of the SWP/CVP, rather than to understand the dynamics of smaller-scale within-system features. Investments of the magnitude considered for in-delta storage require detailed analysis but the need for analysis in a timely manner usually means application of established modeling tools. However, the models, and all other investigative approaches, must work to reduce the current level of scientific uncertainty, and thus, the risks associated with such a project.

In addition, because of the need for public understanding of complex technical issues and the need to be clear concerning what the models can and cannot simulate, planning studies such as this can be more useful if they explicitly refer to, and perhaps summarize, critical parts of model documentation. Reviewers repeatedly indicated the need to understand how the models work in order to fully assess their output. For instance, it would have been helpful to understand the decision-making process for flow allocation at nodes within CALSIM, as well as the temperature approach used with DSM2, to assess their use in these studies.

Similarly, stating the level of accuracy and quantitative uncertainty of any models used, as estimated during verification and validation processes, also assists those interested in the planning process in determining how well the model does at simulating average conditions, extreme events, daily fluctuations and interannual trends. In this case, it would also provide an indication of the level of confidence in the model estimates of project water yield and water quality parameters relative to the operational criteria set forth on Decision 1643. Uncertainties (e.g. estimation errors) were generally not quantified in the studies. The magnitude of error for all predictions should be estimated so that, for example, predicted differences between base (no DWP) and DWP cases, which in many instances were very small, can be compared to the size of the error. For example, are the projected benefits of the DWP smaller or larger than the size of the estimation error? If error is larger than the magnitude of expected benefits, then those expected benefits might not be taken seriously. Quantification of uncertainties would also be necessary for evaluating predictions of DOC, temperature, and DO compliance. Validation studies and quantification of estimation errors should be provided for the DSM2, CALSIM, DO, and temperature models. Further, since the output of a modeled scenario may be highly sensitive to uncertainties in the multiple model forcings (e.g. meteorological, geometric, operational), causing propagation of uncertainty and potentially extremely different outcomes, modeled outputs may best be expressed as a *range* of possible outcomes as opposed to one distinct outcome.

It is likely that some of this information is readily available for the models used and could be incorporated into future planning documents. However, it is necessary to also include this level of background detail for any additional existing models which are used or new models that are developed as planning proceeds.

3.2.1 Physical Modeling

CALSIM model

Use of the CALSIM II model as a driver to DSM2 is generally deemed to be a strength since together they appear to be those currently used to assess operations and water management within the Delta, allowing the project to be considered in the context of current delta operations. Some basic description of how the model works would have been helpful in further evaluating its reasonableness for this application. As discussed above, the lack of model documentation made it difficult for reviewers to assess the performance of this component of the modeling approach.

DSM2 model

For the multi-year simulations performed for this study, the computational efficiency and extensive previous application of the DSM2 model to the Delta make it a logical choice. Within the constraints and assumptions of the DSM2 one-dimensional framework, predictions of transport of water and conservative scalars (like EC) are expected to be generally valid; however, quantitative and graphical comparisons of measurements against DSM2 output for a wide range of operational and hydrologic scenarios is necessary to establish reviewers' confidence in the predictive ability of this model. For example, for a range of scenarios, how large is the DSM2 error in predicted water and EC quantities and fluxes? No such model validation information was provided with the review materials; therefore, although it is expected that the model performs well in those areas, reviewers were unable to vouch for the model's quantitative predictions.

One major limitation of the DSM2 model in the context of reservoir water quality prediction is its inability to resolve vertical or lateral variability within reservoirs or adjacent channels. DSM2 apparently treats reservoirs as continuously stirred (internally homogeneous) tank reactors, implicitly assuming that water is never stratified and that water quality constituents never vary spatially within them. In addition to the stratification issue, the irregular topography of the islands suggests horizontal variability in water depth and physical-biogeochemical processes will be present. Reviewers repeatedly called into question these assumptions.

Another limitation of the DSM2 model has to do with its apparent inability to simulate complete drying of reservoir beds and the requirement in some studies that modeled minimum water heights are 0.5 ft. Although operational schemes are unclear about the saturated/flooded condition of the soil post-discharge and pre-refill, implications of how the DSM2 model's limitations in accurately characterizing water depths could impact assessments of water quality, macrophyte growth, etc., should be addressed.

3.2.3 Ecosystem Modeling

Dissolved oxygen and water temperature modeling

The spreadsheet modeling approach taken in predicting DO and temperature (T) likely provide a reasonable start for the process of projecting DO and T compliance immediately in the vicinity of the reservoirs. However, as discussed elsewhere in this report, several simplifying assumptions (some of which the author discusses) may substantially limit the realism of the results. Such simplifications include the use of a daily timestep (instead of a timestep resolving diel dynamics), neglect of potential thermal stratification inside the reservoirs, and the probable assumption of full mixing of reservoir discharges across the adjacent channel cross-section. Further, important biological and biogeochemical processes are not considered in the DO model. Although there are good discussions of processes involving algal growth and submerged aquatic vegetation, of the limited data available to describe such productivity in this system, and of a sound conceptual model of fates of macrophyte detritus, the inability of the current quantitative assessments to embrace such issues is a major shortcoming.

The heat budget equations were openly described and were probably standard but were not referenced or shown to be validated quantitatively. The DO calculation approach was also for the most part unreferenced and presented without validation information. The mass balance approach was not described in detail (no equation was given) but was reasonable within a one-dimensional framework. Although the approaches are likely generally reasonable, the lack of supporting/validating information makes it impossible for the reviewers to really confirm the validity of the results. Little discussion was given of alternative models and their benefits.

Reviewers seriously questioned the use of the SMARTS experimental data in the DO sag term in the model. It was assumed that the SMARTS experiments captured most important DO losses, but no substantiation of this assumption was offered. Further, SMARTS data were used to estimate DO losses for cases of high and low organic carbon substrate. Unfortunately, the high organic carbon substrate also had deeper water, so it was difficult to separate out individual effects of water depth from carbon content of the substrate.

It appears that the so-called “verification” of the DO approach was actually “calibration” since algal growth rates were adjusted so that DO concentrations at Webb matched measurements. In the context of such a “model tuning” exercise, it should not be surprising that predicted Webb DO compares favorably to measurements. It appears a similar approach may have been taken at Bacon as well. If the model was tuned to match observations (a calibration exercise), then we cannot take the results of that calibration as reliable predictions of an independently tuned model. Rather, the model should be calibrated independently and then used (without further tuning) to provide actual *validation* output.

Although the T/DO approach provides a reasonable start to assessing bulk impacts of discharges immediately in the vicinity of the reservoirs, it is insufficiently sophisticated to assess the real impact of the discharged waters on water quality in the Delta. Reviewers suggest the use of three-dimensional models that can account for bathymetric complexities, local stratification, lateral variability, and variability in mixing that will affect the fate of reservoir outflows. In addition, both temperature and DO need to be modeled within the Delta-scale, one-dimensional context of DSM2.

3.3 Assumptions

The reports stated several assumptions with which reviewers had questions regarding validity and impacts on results. Examples are:

- The assumption that DOC is equal to raw-water TOC is clearly invalid and use of this relationship potentially can have tremendous repercussions on water utilities because regulations are based on TOC rather than DOC. Routine instrumental techniques to analyze TOC produce erroneous results and the need to separately measure DOC and POC to calculate TOC was emphasized by one of the reviewers.
- Another significant invalid assumption is treating DOC as a conservative constituent in the channel waters. The same biological productivity considerations discussed in detail above, also apply to channel-water DOC-carbon cycling and need to be explicitly addressed.

- Most studies assumed the water column was well mixed (i.e. not prone to vertical density stratification). Many reviewers questioned the validity of this assumption and remarked on the multitude of critical quantities and processes (such as dissolved oxygen, organic carbon, phytoplankton, macrophyte, and mercury dynamics) that could impact or be impacted by the very possible development of temperature stratification within the proposed reservoirs and possibly adjacent channels. In general, very little consideration was given in the reports to the implications of this assumption on the net water quality of reservoir discharges.
- Horizontal variability within reservoirs was not considered either. Reviewers expect that horizontal variability in physical and biogeochemical processes may---and probably will---develop due to variations in bathymetry, biases in wind direction, and development of secondary flows in corners and coves. Therefore, we cannot expect that the reservoirs would function as Continuously Stirred Tank Reactors; however, the predominant assumption in these studies was that they would.
- The DICU model, used to project consumptive uses in the Delta for 2020 level of development, does not incorporate any change in consumptive use associated with the project but rather redistributes without-project levels of use across the Delta.
- A 2020 level of demand and hydrology was assumed for the project instead of an extreme case (e.g. 2055) or a reasonable stepped progression through time. Extreme climate scenarios (e.g. El Nino, La Nina, extended droughts) should be considered.
- It was assumed that the SMARTS tanks incorporated (almost) all of the critical DO loss processes, so the DO “sag term” was based on SMARTS data. However, no specific discussion was offered of the universe of critical DO sinks and relevant processes (e.g. due to vascular plants, detritus, animals, etc.) potentially present in the reservoirs and specifically which of those the SMARTS tanks captured. Reviewers were skeptical of this simplifying assumption.

These examples (more are contained in the individual detailed reviews) emphasize the need to assess, test, and justify the validity and credibility of assumptions made throughout the reports.

3.4 Recommendations – Scientific Validity

Conceptual model of carbon

The reviewers strongly recommend development of a comprehensive, process-level, mechanistic-based conceptual model of the carbon dynamics in the reservoir system, specifically including release of DOC from peat soils, biological productivity, and the carbon dynamics and cycling processes associated with these carbon sources. The conceptual model needs to consider carbon sources, sinks, and biogeochemical processes affecting and controlling carbon quantity and quality in the system. Furthermore, the carbon cycling conceptual model needs to be integrated with the hydrologic and hydrodynamic frameworks driving carbon transport in the system.

Measure DOC flux from peat soils

Measurement and modeling of diffusive fluxes of DOC from reservoir soils using either intact soil cores or *in situ* mesocosms will provide valuable information regarding contributions of DOC from the peat soils. Replication and judicious selection of sites will aid the evaluation of both uncertainty estimates and spatial variability. Additionally, monitoring of gaseous carbon fluxes (CO_2 and CH_4) in the cores or mesocosms should provide information on the importance of microbial processes influencing DOC release.

Modeling reservoir water column DOC

An alternative to the logistics-equation approach for modeling water column DOC could take on the form:

$$DOC_{\text{water column}} \text{ or } \frac{dDOC}{dt}_{\text{water column}} = \textit{sed-water flux contribution}$$

+ water column vertical diffusion and mixing contribution

+ water column production or transformation contribution (function of k)

+ horizontal exchanges or flushing contribution,

with the relevant biogeochemical, hydrologic, and hydrodynamic processes contained within each term.

Modeling seepage return

The reviewers recommend the use of a 3-D model for estimating seepage returns for the complex peat soil-reservoir system. In addition, the importance of understanding the interactions between the reservoir surface water and the local and regional groundwater systems, as well as using a more realistic groundwater DOC value, need to be incorporated into the model to better reflect the hydrologic complexities of the system.

Need to consider effects of photooxidation on DOC quality

An important process that needs consideration in evaluating DOC dynamics is the potential impacts of photooxidation on organic matter quality in Delta waters. Photooxidation of DOC is not a simple issue. It appears that photooxidation has the impact of making organic matter that is refactory to biological degradation (such as humic substances) more biodegradable, and making biologically labile constituents (such as algal exudates) less biodegradable.

Need for common scenarios and assumptions

A common observation of reviewers was the need for improved coordination between study components. A remarkable breadth of issues was covered in the studies, but in many cases the separate studies were based on different sets of operational, geometric, and hydrologic assumptions. For example, the Biological Productivity Study was based on the assumption of steadily filling reservoirs over a 3-month period; whereas DSM2 simulations were based on filling scenarios occurring over maximum 1-2 months. Other inter-study discrepancies occurred with respect to the depth of water in the reservoirs after release and the possibility of refilling within one year. Future studies should all be based on the same operational, geometric, and hydrologic scenarios.

Three-dimensional modeling

Due to concern over the possibility of vertical temperature stratification within the proposed reservoirs and possibly in adjacent channels, it is recommended that a three-dimensional hydrodynamic model be applied to the proposed reservoirs and adjacent channel environments and include components for heat flux and transport, wind-induced turbulent mixing and residual circulation, wetting and drying of computational cells, spatially variable bathymetry, and transport capabilities for embedded reactive constituents. Such a modeling approach would be implemented for

1) projecting and understanding the detailed physical interactions between wind, geometry, surface heating, inflows, and outflows in the proposed reservoirs;
2) ultimately studying the potential effects of those physical interactions on important physical, chemical and biological quantities such as EC, TOC, DOC, DO, TTHM, bromide, UVA, chlorophyll a, and temperature;
3) studying cross-sectional variability and mixing in channels adjacent to reservoir islands,
4) comparing with DSM2 results to identify regimes when a one-dimensional approach is appropriate, to generate error bars on the DSM2 estimates, and to refine the representation of key processes within DSM2,
5) refining the placement of reservoir intakes and discharges; and
6) potentially merging (if feasible) a three-dimensional representation of reservoirs and adjacent channels with the DSM2 one-dimensional representation of the greater Delta. Also recommended is application to the proposed reservoirs and adjacent channel environments of a three-dimensional hydrodynamic. Associated measurements resolving vertical and lateral profiles of hydrodynamic quantities as well as chemical and biological constituents are recommended for calibrating and validating the multidimensional, integrated model.

Other issues which a multi-dimensional hydrodynamic model could inform include: 1) effects of seepage pumps on internal circulation and residence time relative to SMARTS tanks and associated implications for water-peat contact and DOC flux; 2) effects of perimeter seepage pumps on adjacent channel hydrodynamics; and 3) forces of discharge on levees bordering *islands adjacent to reservoir islands (i.e. for ultimate stability evaluation of adjacent island levees).*

The hydrodynamic base of a 3D modeling effort would not have to be built from scratch. Examples of existing, verified, and potentially appropriate 3D hydrodynamic models include RMA-10 (Resource Management Associates, Inc.), ECOMSED (Hydroqual, Inc.), TRIM3D or UNTRIM (Prof. Vincenzo Casulli), and Delft3D (Delft Hydraulics).

Ecosystem Functions and Process integration

Because many of the modeled biological and chemical constituents have potentially large effects on other constituents (currently modeled separately), it is recommended that their dynamics be studied in tandem and in a mechanistic manner. For example, water temperature was studied separately from DOC, TTHM, and bromide. Because 1) DOC transformations are sensitive to temperature, 2) TTHM and bromide depend directly on DOC, and 3) other biochemical processes related to biological productivity and the carbon cycle such as algal growth and dissolved oxygen relate to most of the above constituents, it is suggested that these all be modeled and studied simultaneously within one model, with the same sets of hydrological, physical, and operational assumptions in place for all constituents and with mechanistic feedbacks between constituents explicitly incorporated and permitted. Similar process relationships exist and should be studied between vascular plant growth/decay, mercury dynamics, and the above processes. Admittedly, understanding and thus modeling ability may be limited for some key processes such as macrophyte growth and mercury transformations; however, the state of the knowledge should be used to at least *bound the range of possible outcomes* as functions of other more “modellable” processes.

Collaboration between multidimensional hydrodynamic modelers and fish biologists is recommended for projecting impacts of changes in flow and transport on sensitive populations.

Fingerprinting for partitioning of reservoir releases and organic matter sources

The use of the DSM2 model in the “fingerprinting” exercises for water source tracing is deemed a very worthwhile exercise. It was suggested that a similar approach be used for quantifying the partitioning of reservoir discharge flows and constituent fluxes between various destinations (e.g. pumps, Bay), as qualitative statements were made about such partitioning but no quantitative work shown.

Analytical techniques are currently being developed that show promise for identifying different sources of organic matter contributing to DOC. Some of these techniques rely on *in-situ* optical measurements that could be tested and suitably applied to the reservoir islands and surrounding channel waters.

Time scales, spatial scales, and time frames of study components

Water quality predictions were generally produced in the form of monthly averaged values, which smoothes out extremes and probably underestimates noncompliance events; whereas, compliance monitoring will presumably not be based on smoothed monthly averages. DO and temperature predictions were performed at a higher daily-averaged resolution, but those quantities may experience extreme diel variability. The time of day of sampling or reservoir release could thus easily determine whether water quality violations occur. Therefore, the day- or monthly-averaged model output so prevalent in most of the studies is generally not appropriate for predicting compliance (this limitation was acknowledged by several of the study authors). Water quality predictions need to be calculated at the time scales of expected monitoring and of the relevant controlling mechanisms (e.g. the daily heating cycle).

Further, the time frames of critical biogeochemical processes relative to expected periods of reservoir release must receive attention. For example, high growth rates of micro- and macroalgae will likely occur during the warm summer periods, potentially resulting in elevated DOC levels; such high-DOC periods could coincide with desired reservoir release times, potentially precluding releases on the basis of water quality violations.

In addition to resolving the time of monitoring and reservoir release, modeling studies should also resolve the location. For example, dissolved oxygen concentration, water temperature, and other water quality constituents such as algal concentration and consequently DOC may vary substantially over the depth of a water column. The vertical placement of samples or reservoir releases could easily determine whether water quality violations occur. Further, water quality may vary substantially in the horizontal, since reservoirs will not be subject to the homogenizing effects of tidal mixing. Residence time of reactive solutes and particles will likely vary spatially within reservoirs, thus affecting net transformation rates and, ultimately, concentrations. Therefore, compliance with water quality restrictions may depend on where *horizontally* monitoring and discharge are performed. The depth-averaged and laterally averaged DSM2 approach may not be appropriate for predicting compliance where and when vertical or horizontal variability is expected to be substantial. A three-dimensional model may help identify scenarios for which the DSM2 structure is appropriate and cases for which it is not.

Finally, it was not clear whether temporal or spatial variability were considered when measurements were used to drive models or compare with model results. For example, is point data from an incompletely mixed tank reactor (SMARTS experiments) or from the possibly stratified Stockton ship canal (for temperature) appropriate for use with the depth-averaged DSM2 model? Are new temperature and DO measurements needed at *actual* discharge locations? Are the environments at which DO measurements are available really representative of the environments to which they are applied?

4. Future Work and Next Steps

Successful implementation of the complex in-delta storage project requires addressing the shortcomings and making the enhancements to existing approaches recommended above, and in more detail in the reviews. Generation of new understanding (information) is essential before the project can be fully evaluated. Not all decisions about implementation need necessarily wait for complete scientific knowledge. But some of the shortcomings in knowledge are severe enough that substantial risks exist if decisions proceed without filling these gaps.

The review has identified substantial uncertainties regarding the water quality of the discharges from the project. The review has documented inadequate consideration of the processes controlling DOC concentration, DO levels and water temperature, all of which are important to the viability of the project. It is paramount to know how likely it is that this project will meet the operational criteria laid out in State Water Resources Control Board Decision 1643. Implementing the project before these issues are more fully addressed poses great risk for the quality of water in the lower Delta and for the operators of the project who may be left with reservoirs full of water that cannot be released because of water quality criteria.

Reviewers also pointed to the great uncertainties regarding the effect on the migration and production of critical populations of fishes, the need for better understanding of project operations on mercury methylation, and the potential role of exotic species in altering system function in the future. These issues also need to be addressed in order that the full implications of the project for the Delta can be assessed, although their immediate implications are probably less severe than those for water quality.

Research should be targeted towards these and any other critical process linkages that the conceptual models show as being sensitive and of high uncertainty. Such research is called for under the adaptive management approach to ecosystem restoration adopted by CALFED. Research should be focused to reduce uncertainty and thus elucidate or improve the conceptual models of the system that assist in determining project benefits or impacts.

Screening of diversions to prevent fish mortality is a common practice. Reviewers expressed concerns regarding the design of the screens and these must be addressed as the development and evaluation of the project moves forward. However, before the standard current agency-approved designs are incorporated by default, a full evaluation of the potential effects, positive and negative, of screening these diversions should be undertaken. The size, number and placement of diversions should be examined relative to the efficacy of screening options, the operational criteria concerning Delta smelt outlined in Decision 1643, and likely variations in the magnitude and timing of diversion relative to changing river flow conditions in relation to anadromous fish use of the adjacent channels. Such information can then be used to optimize the design of the diversion configuration to minimize damaging effects on fish while allowing operation of the storage facilities to proceed.

Another crucial aspect of project implementation under adaptive management is monitoring, not simply to validate project expectations or meet regulatory constraints on system operation, but to improve understanding of the project in operation. Reviewers note that shallow aquatic ecosystems are increasingly reported as switching from one persistent condition to another. These transitions can be driven by alterations in nutrient supply, shifts in climatic conditions, or introductions of exotic species. Such state transitions can be associated with substantial changes in ecosystem function. Monitoring must be sensitive enough to identify these changes, and be used to modify conceptual models appropriately.

Currently, SWRCB Decision 1634 calls for the completion of a compliance and monitoring report to include:

> 'A detailed and comprehensive monitoring program for the periods when the DW Project is discharging water that identifies parameters to be monitored including chloride, bromide, electrical conductivity, dissolved oxygen, modeled channel flow rate, discharge rate, total dissolved solids, turbidity, dissolved organic carbon, UVA, total organic carbon, and water temperature; sampling locations; sampling frequencies; analytical methods; and quality assurance/quality control procedures in accordance with the analytical methods defined in the SDWA regulations; (40 CFR § 141.135(b).)'.

This list should be expanded to include local meteorological data, vertical hydrographic profiling to assess stratification, detailed monitoring of seepage returns to assess their influence on water quality. These parameters must be assessed at sufficiently detailed temporal and spatial scales to drive the numerical models that govern operation, as well as to provide insights into system function. Further, since reservoirs will most likely not operate at steady state, rendering conditions during discharge periods a function of antecedent conditions, monitoring should be expanded temporally to include periods *between* discharge periods so that potential hindrances to discharge may be understood and effectively managed.

Considering all the issues above, and their relative risks, the following steps are recommended (Table 1) to reduce uncertainty about whether the project is likely to meet the water quality criteria controlling operation, and provide a sound scientific basis for making a decision regarding project implementation. The steps are presented as Tasks on a timeline to illustrate how they develop information to elucidate project dynamics and build towards a more complete assessment of how the project might operate under the Decision 1643 criteria and the variations in both conveyance operations and environmental drivers.

Table 1. Next Steps by Task and Timeline

Task	Year 1	Year 2	Year 3	Year 4	Year 5
1. Detailed Conceptualization to include detailed DOC conceptual model	xxxxxx				
2. Develop 3-D hydrodynamic model of reservoirs and adjacent channels inc. necessary data collection.	xxxxxx				
3. Empirical measurement of DOC fluxes from peat soils from reservoir islands inc. spatial variability	xxxxxx				
4. Technical forum to present current status of scientific understanding and consider next steps	xxx				xxx
5. Develop model of processes controlling biological prod. within reservoirs.		xxxxxxxxxxxxx			
6. Monitoring of biogeochemical processes in existing Delta habitats.	x x x x x x x x x x x x x x x x				
7. Develop model of processes controlling DOC within water column.			xxxxxxxxxxxxxx		
8. Develop climate change and variability scenarios to include extreme conditions.		xxxxxx			
9. Integration of physical modeling tools.		xxxxxx	xxxxxx		
10. Modeling of reservoir operation.				xxxxxxxxxxxxxxx	

Task 1. Detailed Conceptualization to include detailed DOC conceptual model: This Task would include the development of a conceptual model showing the processes, and their linkages, both driving project operation and affected by project operation. Because of the complexity of the system within which the project is set, a series of nested conceptual models is recommended: the water conveyance system (largely hydrologic, considering EC and operations), the delta (including ecosystem and water quality considerations), and the reservoirs/channels (including detailed consideration of DOC and ecosystem processes).

Task 2. Develop 3-D hydrodynamic numerical model of reservoirs and adjacent channels including necessary data collection: The need to address the potential for stratification within the reservoirs has been repeatedly noted by the reviewers. This effort would include vertical and horizontal variability within the reservoir islands and the adjacent channels. Data collection to establish local boundary conditions in the channels and to validate the model would be part of this Task.

Task 3. Empirical measurement of DOC fluxes from peat soils from reservoir islands inc. spatial variability: Reviewers repeatedly noted the need to assess DOC fluxes from peat soils from the reservoir islands in order to incorporate specific character of substrate (e.g., porosity, structure and organic content) and to evaluate in more detail the DOC dynamics of the islands, including these fluxes. Although empirical flux measurements using in-situ chambers cannot account for changes in flux associated with water movement across the substrate, chamber data can provide some estimate of the diffusive DOC fluxes from the substrate under the relatively quiescent conditions expected near the reservoir bed during maximum storage conditions, and an appropriate model of these processes can be developed as part of the water column model (Task 7). Measurement of gaseous carbon fluxes as part of this task also will provide insight regarding the role of soil microbial processes in DOC release.

Task 4. Technical forum to present current status of scientific understanding and consider next steps: Recognizing that these first three Tasks will address some of the most critical uncertainties regarding project operation under water quality criteria, that understanding of delta dynamics, water quality issues and ecosystem processes is developing rapidly, and that some ongoing studies may not have been considered in this review, this Task provides a mechanism for scientists, regulators, managers, operators and stakeholders to be informed of the current scientific understanding of the issues surrounding in-delta storage. The participants will review existing and newly developed information and suggest modifications to Tasks 5 through 10 as appropriate. A second technical forum is recommended when all the Tasks are completed.

Task 5. Develop numerical model of processes controlling biological productivity within reservoirs: The reviewers have indicated the importance of considering primary production (both algae and macrophytes) in assessing DOC production and DO levels within the reservoirs. Numerical models should be developed to allow these processes to be considered in the dynamic context of reservoir operations. The conceptual model (Task 1) will drive the processes incorporated in the model, and the importance of some factors (such as nutrient availability, turbidity and grazers as factors controlling algal primary production) should be assessed using sensitivity analysis prior to the development of detailed dynamic model components in the context of the 3D hydrodynamic model.

Task 6. Monitoring of biogeochemical processes in existing Delta habitats: Some analogs for the reservoir islands exist within the Delta, although they are mostly subject to tidal exchanges (which will not be the case for the reservoirs). In particular, Twitchell Island wetland restoration experiment areas (not subject to tidal exchange) provide examples of shallow flooded conditions (an analog for low water conditions within the reservoirs) and the southern part of Mildred Island is a relatively deep flooded area within limited tidal exchange. Monitoring of biogeochemical processes should be conducted in these areas to develop a context for the varying conditions reservoir islands might experience during flooding and discharge cycles. In addition, monitoring of biogeochemical processes in the channels adjacent to proposed reservoir islands will provide data to validate modeling of that area and the translation of information from Twitchell and Mildred to the proposed project location.

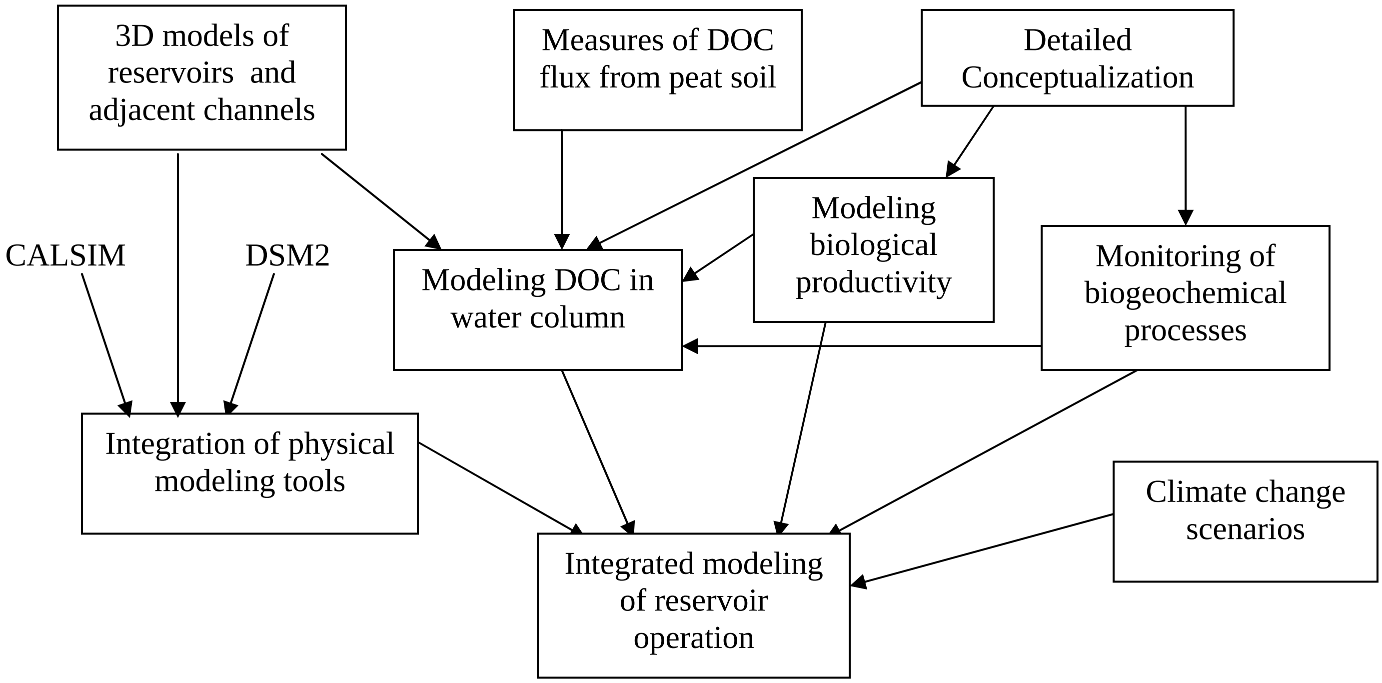
Task 7. Develop numerical model of processes controlling DOC within water column: This review has provided a conceptual basis for modeling DOC within reservoir waters, including the roles of flux from peat substrates (Task 3), and ultimate incorporation into a model of three-dimensional circulation within the reservoirs (Task 2). Data from monitoring in Task 6 and the empirical studies in Task 3 will be used to build and validate the model. Before incorporation of DOC processes into the full 3D model, initial development and verification within a one-dimensional vertically resolved framework (i.e. without net horizontal transport) may be helpful.

Task 8. Develop climate change and variability scenarios to include extreme conditions: Current and future variability in climate, including global warming trends and cyclic phenomena such as El Nino-La Nina, have a great influence on the availability of water within the Central Valley. Reviewers acknowledge the current limited incorporation of these factors into the evaluation on in-delta storage. This Task would develop future scenarios of climate change and variability (e.g., precipitation and temperature regimes) that would provide a range of water availability conditions within which in-delta storage dynamics can be assessed, including extremes of water surplus and water deficit, as well the influence of increased air and inflow temperatures and modified hydrographs on reservoir circulation and biogeochemical dynamics.

Task 9. Integration of physical modeling tools: The review recognizes that most of the models currently used in the studies are designed at assist operational decisions rather than to be used in detailed planning assessments. However, it is also acknowledged that to the extent possible currently available and widely understood modeling tools should be used in the assessment. This Task develops mechanisms for integrating the newly developed, smaller scale, reservoir-specific three-dimension models (Task 2) with those that address system-scale water operations (CALSIM) and Delta dynamics (DSM2). The goal is to use output from the larger scale models to drive the smaller scale models, and provide for the necessary iterations and feedbacks to ensure that the physical dynamics of in-delta storage can be considered quantitatively in the context of the Delta and the whole conveyance system.

Task 10. Modeling of reservoir operation: Tasks 1 through 3, and 5 through 9 (as modified during the Task 4 technical forum), build information, technology and understanding towards the point where models can be used to fully evaluate in-delta storage operations in the context of the water quality discharge criteria required by Decision 1643 (Figure 1). This Task involves the development of an integrated modeling approach to incorporate the information derived from previous Tasks and our conceptual understanding of in-delta storage in a broader systems context. Extension of the DSM2 to include temperature, DO, and/or DOC dynamics in channels and the greater Delta may also be necessary. Using this model, agencies, stakeholders and operators will be able to evaluate project performance under a range of conditions (including climatic extremes) and make more informed decisions regarding the risks involved and potential benefits derived from proceeding with the Plan.

Figure 1. Linkages among Tasks outlined in Table 1 and described in the text.



The timeline in Table 1 has been derived based upon consideration of available approaches and expertise and the challenges involved in some of the Tasks. The goal of these recommendations is to move towards an informed decision on in-delta storage implementation. To expedite this process and meet the proposed timeline it is expected that DWR will make use of the best available expertise in the various fields of science and will call upon their in-house personnel, consultants, and both in- and out-of-state experts to move these Tasks to fruition on the proposed timeline. While this might be accomplished using a competitive RFP process, the need to move forward with these Tasks and to provide integration of models and information developed in different Tasks requires a more focused approach to the selection of those who can best accomplish what needs to be done. Thus, it is recommended that a Steering Committee of independent advisors (i.e., experts not directly involved in accomplishing any of the Tasks) be convened to advise DWR in the selection of study participants, to review draft reports, and recommend modifications of these Tasks and/or the timeline as appropriate.

SECTION – D

In-Delta Storage Program
State Feasibility Study

May 2002 Reports Reviews by Individual Panel Members

SECTION D: MAY 2002 REPORTS REVIEWS BY INDIVIDUAL PANEL MEMBERS

Review #1

21 July 2002

Dr. Sam Luoma
CALFED Bay-Delta Program
1416 Ninth Street. Ste. 1148
Sacramento, CA 95814

Re: Review of CALFED/DWR In-Delta Storage Program

Dear Dr. Luoma,

As the first step in my review of the CALFED/DWR In-Delta Storage Program, I have read fully the following draft reports: Environmental Evaluations, Water Quality Investigations, Water Quality Modeling Technical Appendix, Synthesis of Data of Reservoir Island Organic Carbon Model, Operation Studies, and Summary Report. I examined these documents in the context of my primary charge to evaluate whether the scientific community would view these studies as valid, at the state of the science, and as useful to managers. First, I offer comments specific to portions of each report and, second, respond to the general questions posed to the CALFED Science Panel.

In contrast to most scientific reports, the documents under review were long and redundant, presented only partially the methods but included many graphical and tabular results. Given the total costs to be incurred if the proposed construction were to be completed, the depth and scope of the scientific analysis is wanting throughout. Although many questions could be raised over minor points, only main issues are highlighted.

Environmental Evaluations: The analyses of environmental impacts of the in-delta storage plans did not consider ecosystem functions. Modern environmental science concerns ecological and biogeochemical processes, such as elemental inputs, losses and recycling, primary production and decomposition, fate and transport of pollutants, and food web interactions. Consideration of these processes is almost completely lacking from the report. Without information on such processes, forecasting changes associated with in-delta storage is not possible.

One peculiar aspect of the evaluation is the perspective that “two significant and unavoidable land use and agricultural impacts” are that prime agricultural land would be converted to water storage and habitat. From an historical perspective, the delta lands under consideration would be wetland habitat if they had not been diked and drained. Although creating a water storage reservoir is a further alteration, restoring wetlands would not seem to represent an impact from an ecological perspective.

The botanical resource evaluation requires updating with current photographs and sampling; photographs from 1987 are not adequate. If habitat types were mapped with a resolution of approximately one acre, why does table 3.6 list areas to a tenth of an acre?
The assertion that little aquatic vegetation, including exotic species, is expected in the reservoir islands (p. 43) is difficult to accept.

Aquatic resources include only fish. Surely, other species are part of the aquatic ecosystem. The summary of aquatic resource issues and impacts (page 50 and 51) is qualitative; cannot more quantitative points be made?

The discussion of temperature and dissolved oxygen (pages 52 to 55) is not satisfactory. Well developed models of vertical mixing are available for the prediction of the likelihood of stratification or mixing. It is not apparent that such models were used. As noted in the report, the CALFED dissolved oxygen model works on a one-day time step, but important fluctuations in dissolved oxygen occur hourly. Clearly, additional studies of dissolved oxygen are warranted.

As discussed (pages 55 to 57), methyl mercury is likely to be formed in the reservoirs, but the dynamics and extent of this process have not been properly examined. In light of the large residual mercury sources in Sierran waters draining to the Delta, this issue must receive further attention.

The hazardous material assessment notes that actual measurements of soil samples from potentially contaminated sites are lacking.

The habitat management plan focuses on species and does not consider ecosystem functions.

Water Quality Investigations: Water quality modeling did not consider nutrients or metals. Nutrient supply is directly related to plant growth, which, in turn, influences DOC levels and ecosystem functions. Metals, including mercury, could impact the uses of the water.

The modeling studies of DOC and other water quality indices indicate probable negative impacts on water quality. Although further work is warranted, the available data do support the conclusions.

The water quality field investigations concerned dissolved organic carbon and incorporated experimental results described in detail in a companion technical report. Experimental determinations of DOC release from peat soils were used to produce an empirical equation for DOC concentration. Hence, the application of the equation is constrained to the range of conditions included in the experiments conducted in the SMARTS facility. While a reasonable approach, these experiments were limited in time and could not represent the full extent of environmental conditions likely in the delta reservoirs. Further work incorporating a more mechanistic evaluation of the production of DOC would be a valuable complement.

The biological productivity studies are largely hypothetical as they are derived from basic assumptions and calculations, not extensive measurements. Since algal and macrophyte growth and decay are well known sources of DOC and can alter the dissolved oxygen dynamics, further studies including actual measurements are required.

Operation Studies: While a subject of on-going investigation, the potential for climate changes to alter the timing and amount of runoff entering the delta and sea level and associated storm surges is large and likely. The operation studies included only a short cursory examination of these potential impacts. It is essential that an engineered system designed to last decades incorporate a more comprehensive analysis of climate changes.

Responses to Questions poised to CALFED Science Panel:

Overall, the studies are empirical and derived from incomplete information about the system of interest. Hence, the forecasts about likely impacts of the proposed schemes are limited in their generality and validity. Fortunately, further work is recommended frequently, but it is unclear if such work is likely. In fact, a proper evaluation of the proposal will require new and different data (see below), more mechanistic models and a more rigorous analysis of uncertainties.

As noted above, the evaluations are deficient in their consideration of most biogeochemical and ecological processes that are central to ecosystem function. In turn, these processes are related to changes in plant growth and decay, and DOC, dissolved oxygen and methyl mercury dynamics. Predictive models must be built on mechanistic understanding of the processes involved.

If the project proceeds, a comprehensive monitoring program should be implemented. Automated recording of meteorological conditions are needed to drive models of stratification and mixing and associated changes in dissolved oxygen and temperature. Profiles of temperature and dissolved oxygen should be a regularly determined. Daily measurements of DOC are critical to ensure the exported water is useable. A model of aquatic plant growth and decay should include periodic measurements of biomass, productivity and decomposition, and will require monitoring of nutrient and light supply. Toxic compounds such as methyl mercury must be monitored.

Studies of shallow aquatic ecosystems are increasingly reporting that these systems can switch from one persistent condition to another. The transitions can be driven by alterations in nutrient supply, shifts in climatic conditions, or introductions of exotic species. The new states usually have significant differences in ecosystem function. Therefore, the managers of delta reservoirs should expect surprises.

Review #2

8 July 02

Subject: Scientific Review for "In-Delta Storage Program"

The CALFED Bay-Delta Program has a complex and multi-faceted mission. The SF-Bay's inland Delta provides a remarkable array of vital ecosystem services to the entire state of California (CA). Transfer of freshwater is central to the economic and population growth of the state of CA; and in this regard, the SF-Estuary's Delta is the *heart* of CA's water conveyance, economy, and population expansion. The concept of the SF-Delta functioning as the CA-state's heart is abstract, and likely under-appreciated or unrecognized by the majority of the state's ~35M residents.

The In-Delta Storage Program was identified in the CALFED ROD as one of five potential surface water storage projects. CALFED and DWR have provided a detailed planning study to evaluate if potential in-Delta Storage Projects are feasible in terms of engineering and economics and to determine if they meet CALFED water quality/quantity and restoration needs. An engineering design review has been conducted by a separate review panel. Previous reviews by several agencies have concluded that the Delta Wetlands Projects "are generally well planned", but requires additional analyses before it is appropriate to invest such a large amount of public funds. Here I provide a scientific and technical review regarding the general limnology and ecology aspects of the planning study primarily in terms of the CALFED Bay-Delta Ecosystem Restoration Program.

The Delta, in many ways, can be thought of as the *heart* of California. This heart integrates a major portion of the state's freshwater and transfers and pumps this water throughout the state. Efforts to modify such a critical feature in the state's water transfer system will meet great public scrutiny. Modification of the Delta's valves, chambers, arteries, and veins would be greatly simplified if the goals were limited to physical hydrology decisions. The layering of water quality, biological, and ecosystem restoration considerations on-top of conveyance issues greatly complicates the overall evaluation of any proposed large-scale manipulations of the Delta's waterways and open water habitats.

The In-Delta Storage Programs aim to increase water supply reliability, improve operation flexibility, and allow water to be conserved during wet periods. In theory, this water diversion and storage could benefit the Delta's ecosystem restoration efforts by allowing greater flexibility of water flow operations to help minimize negative impacts on fish migrations. Overall, I recognize the potential value of In-Delta Storage Programs and support the general effort of the proposed project; however, I have several major concerns regarding the present proposal.

Major concerns:

1) The Delta Wetlands Project has not integrated proposed landscape modification with an Adaptive Management plan. Adaptive management is the flexible foundation strategy being employed by the CALFED Bay-Delta Ecosystem Restoration Program. Adaptive management is the conceptual basis underlying other major ecosystem restoration programs; including, the Florida Everglades, Colorado River in the Grand Canyon, and Columbia River. Critical focal species or threatened populations are key dependent variables in all four of these large restoration projects. Ecosystem processes, environmental stressors, and habitat attributes that are known to influence critical species are equally valued in these large projects. Adaptive management REQUIRES thoughtful, detailed experimental designs that enable change in populations or ecosystem processes to be detected following the manipulation of habitats and landscapes.

Any In-Delta Storage Program must be carefully integrated with the Ecosystem Restoration Program's broad adaptive management strategy.

2) Criteria for gauging broad public acceptance are not explicit. Benefits and ecosystem services are discussed in a qualitative manner. The public will want to know exactly what is gained with as much as a $1.1 Billion dollar investment. Technical and financial feasibility were investigated, but the direct contribution to reducing conflicts among potential water diversions is abstract. Would the storage areas serve as giant pies of water with diffuse use? Will the habitat areas be carefully connected to the needs of the Bay-Delta Ecosystem Restoration Program? Again, there is a need for designing these projects in terms of "large-scale experiments" in which the enhancement of specific ecosystem services are evaluated in a quantitative manner. This would provide the public with the scientific data that could be used to help make the important societal decisions regarding future water allocation and landscape modification.

3) The In-Delta Storage Program is heavily reliant on modeling efforts. Not surprisingly, much of the materials provided in support of the proposed storage facilities and shallow-water habitats are based on a series of engineered models. This initial modeling approach needs to better balanced with a plan to gather the most important and useful empirical data during all stages of construction and implementation. Empirical data are required to validate the array of assumptions used in constructing these models AND to ensure the Program has embraced the adaptive management approach to learning about ecosystem response. The use of words such as "gaming" and "subjective judgments and observations" begs for clear fusion of modeling and empirical data collection plans.

Detailed comments:

*Why did the DSM2 Water Quality Model NOT include DO and temperature?

*The summary findings from the engineering design review were troubling and suggest fundamental design problems.

*I strongly support the recommendations concerning the Operation Studies. It is not clear how the water will be divided. Further, the project's effect on critical populations of fishes (whether beneficial or detrimental) is poorly understood.

*The role of the peat soils in contributing dissolved organic carbon with a high fraction of humic acid to the storage water is poorly understood. These materials provide problems for drinking water quality. This is a major issue for the CALFED Bay-Delta Program. Long-term trajectories of these soil/water column interactions, especially at the scale proposed, are unknown.

*I strongly support the recommendation for alternate fish screens. Even small fish kills or entrapment would result in serious public relations problems. In addition, there are repeated concerns about fisheries criteria application.

*The presentation of multiple management options begs for integration with adaptive management strategies. Again, a need to embrace large-scale EXPERIMENTATION.

*The "Fatal-flaw" list for excluding islands in 2.2.5 is very important and could be used to support the selection of the various tract combinations to the public. There are a limited number of options available. This is an important piece of information for everyone commenting on the project.

*The DOC concentrations discussed in 3.5.4 are very high. Have soil pore water concentrations or small agricultural return concentrations been used to generate these assumed DOC values? They seem to be an order of magnitude too high for the Delta waterways, but maybe these values do reflect water laying over peat soils. Such high DOC values, with high concentrations of humic materials, will certainly be a concern for drinking water quality.

*The state of CA and Delta are likely highly sensitive to subtle shifts in temperature and weather patterns in response to global climate change scenarios. Water storage and conveyance concerns will almost certainly change as snow pack and snow melt patterns vary in the Sierras. Delta storage volumes are small relative to snow pack volumes, so the connection between climate-change and in-Delta storage operations seem to be somewhat disconnected?

*TOC, chloride, DBPs, DO, and Temperature have been identified as critical chemical features of the Delta's water quality. They are all essential dependent variables within careful experimental designs aimed at monitoring and detecting environmental change in response to the Delta's manipulation. Essential additions to this list should include:

1) Chlorophyll a: Phytoplankton is known to be coupled to the Delta's planktonic foodweb, hence phytoplankton biomass (i.e. chlorophyll a) is an important dependent variable for gauging the ecosystem response.

2) Suspended particulate matter (SPM): Primary production in the Delta is light limited, hence SPM data can be used in site-specific empirical models along with chlorophyll a to predict phytoplankton primary production. Further, SPM is critical to predicting wetland success ional trajectories.

3) Biological oxygen demand (BOD): Standard 5-day BOD measures can be used to evaluate organic matter lability. The Stockton Deep Water Channel, which is located relatively close to the manipulated islands, experiences large-scale BOD deficits that threaten fish migration and survival.

* I strongly support the recommendation regarding the DSM2 Models results. Thoughtful studies MUST address “in reservoir” biological activity and impacts on water quality. As currently described, water quality will suffer with the addition of large water storage facilities within basins of peat soils.

*The Delta’s peat soils are unique in regards to similar restoration programs and water storage programs, hence “model” programs cannot be mirrored in the CALFED Bay-Delta Program. The small-scale lab and tank experiments conducted to address this issue were a logical starting point, but large-scale experimentation in the field is required. I strongly support the recommendations regarding additional field experiments in this area of concern.

*The Biological Productivity Studies are insufficient to adequately predict or forecast with any amount of certainty the biological productivity (in terms of plankton, benthos, and macrophytes) of the these large storage facilities. Basic research, closely connected to the adaptive management process, is critically needed in this area. I strongly support all recommendations regarding Biological Productivity Studies and Temp./DO Studies.

*Several of the engineering issues/concerns and subsequent evaluations of environmental resources highlight the uncertainties regarding the migration and production of critical populations of fishes. “Uncertainty” is the key word. In-Delta manipulation must be conducted in a thoughtful, sequential manner in order to decrease uncertainty in terms of impacts on these critical populations.

*Economic analysis is critical to the success of any In-Delta Storage Program. The discipline of ecological economics is in its infancy, and this report presents a first attempt at quantifying an array of Delta-ecosystem uses. Ecological economics is certainly beyond my scientific expertise, but my gut reaction is that the economic analysis of In-Delta Storage needs to be expanded and improved. The quantification of ecosystem services (in terms of $$$) will resonate with the states’ taxpayers.

How can we justify a Billion dollar expenditure of public funds in terms of the state of CA’s water-resources needs? A qualitative description of potential ecosystem services is Stage 1 (this is included in the present proposal). A quantitative forecast based on our emerging global understanding of the value of specific ecosystem resources is Stage 2 (the current proposal is somewhat weak in this area). But… what is missing from such an analysis?

The final Economic Analysis (i.e. Stage 3) must include an economic analysis that incorporates somewhat generic value criteria of ecosystem services weighted with societal values of the state of CA’s taxpayers. How do we weight the value of X-# of Delta smelt?, or Y-# cubic meters of high-quality drinking water, or Z-# of cubic meters of agricultural water? etc… etc… etc…

Broad public education of the Delta's role in the supply of some of state of CA's ecosystem services must be a mandatory component of such large-scale manipulation of the heart of the state's water conveyance system.

Further, all stakeholders and the general public must embrace the need for adaptive management of the Delta's ecosystem. Scientists and managers must provide the public with authoritative and quantitative data sets that enable the public to gauge the Delta ecosystem's response to the attributes that are deemed most valuable by an informed public.

Review #3

REVIEW OF DRAFT REPORTS FOR THE CALFED/DWR IN-DELTA STORAGE PROGRAM

I reviewed the following documents provided by CALFED for scientific and technical merit:

- In-Delta Storage Program Draft Report on Water Quality Investigations
- Synthesis of Data for Development of Reservoir Island Organic Carbon Model in DSM2 Model
- Water Quality Modeling Technical Appendix, Integrated Storage Investigations, In-Delta Storage Feasibility Study

I also reviewed the following supporting documents.

- Levee Stability and Seepage Analysis Report for the Delta Wetlands Project Revised EIR/EIS by URS Greiner Woodward Clyde
- A Trial Experiment on Studying Short-Term Water Quality Changes in Flooded Peat Soil Environments by Marvin Jung and Associates, Inc.

My comments primarily address the analysis, synthesis and modeling of water quality data and seepage relative to the proposed project. In general, the reports do not reflect optimal data collection and analysis and studies lack scientific credibility. In some cases, the results are misinterpreted which resulted in erroneous proposed conceptual models. Specific comments follow.

The In-Delta Storage Program Draft Report on Water Quality Investigations describes the use of DSM2 model for predicting the effects of the project on water quality in the Delta. The report briefly describes the algorithim that predicts changes in DOC and other constituents based on experiments conducted at the SMARTS facility and the resultant equation on page 24. These experiments are flawed and the equation on page 24 probably does not reflect processes occurring in peat soils for several reasons. As described in Jung and Associates (1999), peat soils were collected from the top two feet on Twitchell Island and placed in fiberglass tanks. Data collected by USGS on Twitchell Island indicate that the nature and concentrations of DOC vary with soil depth. The highest DOC concentrations are associated with shallow soil layers. Further, removing and mixing the soil increases the surface area for contact with interstitial water relative to field conditions.

The SMARTS tanks create a situation in which the peat remains in long-term and greater-than-field-condition contact with peat of the highest DOC Therefore, the equation and predicted results may predict DOC concentrations that are higher than what will occur. The report acknowledges uncertainty and uses high and low bookend values based on tanks with different flooding depths. Jung justified higher values relative to other studies based on a proposed longer residence time on flooded islands and temperature effects However, the soil disturbance factor is also at least partially responsible and cannot be quantified or ignored. Also, there will be circulation due to pumping of seepage wells on the reservoir islands which will tend to reduce residence time relative to the SMARTS tanks.

In situ, the transport of DOC from peat into an overlying water column is due to diffusion and convective movement across the water-peat interface and microbial activity in the peat. Under flooded conditions, microbes probably play a minor role relative to diffusion and convection in contributing DOC to the water column overlying the peat. In the water column, mixing and hydrodynamics process will govern the distribution of DOC concentrations. The SMARTS experiments do not sufficiently address the transport or microbial processes. An understanding of what will happen when the peat islands are flooded requires controlled experiments that reflect field conditions.

In the report *Synthesis of Data for Development of Reservoir Island Organic Carbon Model in DSM2 Model,* Jung (2002) describes the synthesis of data for DSM2 simulations. He presents the biological growth model to explain the increases in DOC concentration over time. He does not consider the alternate hypothesis that the DOC increase is at lease partially the result of increasing contact over time with the peat which has been increased as the result of soil disturbance. The medium for conduct of the experiments places the results in doubt.

There are other examples of conceptual and data-analysis errors in Jung's 2002 report. Plots of DOC and UVA with time provide some insight into processes affecting these constituent levels over time. However, recent data from Twitchell Island studies show that DOC and UVA in agricultural drainage water are related to the timing of fall and winter rainfall and groundwater level changes on the island. One can plot the data in Jung (2002) such that physical processes are examined relative to the change in constituent levels. As shown in Figure 1 for Bacon, expressing these levels as fraction of a measure of central tendency relative to precipitation provides more insight about seasonal changes. The figure more clearly shows seasonal changes and the impact of precipitation on DOC levels. High DOC values consistently follow winter precipitation. This agrees with the understanding that high groundwater levels on the islands lead to high DOC in drainage water because of the saturation of shallow layers that have higher DOC. Water flows from these layers to drains increasing the DOC concentrations of drain water. Summer increases in DOC shown on Figure 1 are probably due to irrigation which also causes flow to drains from the saturated shallow peat layers with high DOC.

The reasons for high DOC is primarily due to saturation of shallow layers and not increased residence time during the winter as stated on page 34. Data on Twitchell clearly indicates the contribution of shallow layers to drainage water during winter as groundwater levels rise. Figure 2 shows the conceptual model for flow to drains based on geochemical and hydraulic data on Twitchell Island. Measured changes in salinity are similar to DOC; higher salinity values are associated with increased water levels during the winter because the shallow peat layers have higher salinity than deeper groundwater.

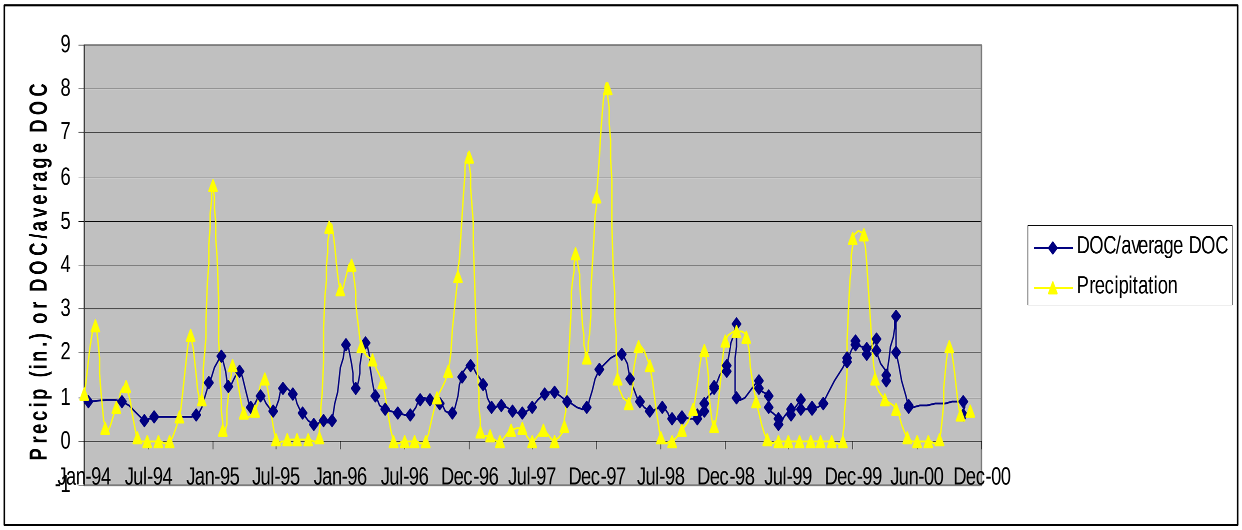


Figure 1. Brentwood precipitation and DOC/average DOC for Bacon Island drainage.

Seepage return estimates described on p. 69 of Jung (2202) and in detail in URS Greiner Woodward Clyde (2000) are problematic. First, there is no available data for DOC concentrations in groundwater underlying the peat. Jung (2002) uses a value of 20 mg/L. Our studies on Twitchell suggest lower values for groundwater in the formation immediately underlying the peat; the median value was 5 mg/L.

Second, the seepage analysis may provide erroneous values for volumes for seepage wells. Specifically, the URS model appears to suffer from over constrained boundary conditions is not three-dimensional and has not been adequately evaluated or validated. The authors established constant head or no-flow boundaries at the vertical and lateral boundaries which may over constrain the model. Sensitivity analysis is needed to evaluate the effects of boundary conditions. Further, the two dimensional flow model may not adequately simulate flow to the wells. Experience on Twitchell indicates that two dimensional modeling is inadequate in peat. The authors indicate that flow to wells may be higher than predicted with higher hydraulic conductivity values. Jung (2002) recognized the high degree of uncertainty in these calculations.

Summary and Answers to Questions for the CALFED Review Panel

1. The reviewed studies have not used the state of the science to estimate DOC concentrations of the reservoir water. The SMARTS facility experiments and the subsequent analyses are plagued by uncertainty about the effect of disturbing the peat soil on concentrations. There is substantial uncertainty in the seepage analysis prediction of flows and DOC concentrations in the groundwater which lead to substantial uncertainty in the loading due to pumping for seepage. There is little or no sound scientific evidence or understanding of processes that will

allow prediction of DOC concentrations. However, it appears that the SMARTS data may over predict the reservoir concentrations.

2. For reasons stated above, the SMARTS experiments and analyses and the seepage calculations do not provide sufficiently reliable results for estimating reservoir concentrations. Further, there is no way to tell how wrong the predictions of concentrations may be.
3. Alternative approaches include 1) measuring concentrations on existing flooded islands such as Mildred Island, 2) conducting experiments to estimate transport and biological processes with undisturbed cores instead of disturbed soils, 3) three dimensional analysis with different boundary conditions of island seepage and pumping, 4) increased sensitivity analysis to determine error in the pumping estimates, 5) sampling to determine DOC levels in the aquifer underlying the peat soils on the reservoir islands, 6) data collection during filling and draining of a reservoir island prior to project approval.
4. Uncertainty analysis is generally absent yet necessary for answering questions about the range of effects of filling reservoir islands.
5. Several key assumptions in the analysis and estimation of water quality and seepage effects on remain unexamined and unquantified. These include the effects of the SMARTS methodology, seepage model boundary conditions and model configuration. Further, the conceptual model for factors affecting DOC concentrations in drainage water and peat interstitial water do not agree with data collected by other researchers.
6. A key knowledge gap is how DOC diffuses or moves convectively into the water column from submerged peat soils. An understanding of this process under field conditions (i.e. with undisturbed soil cores) could provide substantial information about the release of DOC into the overlying water column.
7. If the project is constructed, monitoring of hydraulic and water quality data including reservoir DOC concentrations in the reservoir, pumped water and drainage water are essential to understanding effects. Hydraulic head measurements at frequent intervals ranging from daily to monthly depending on the filling and emptying cycle will be essential for improved seepage and pumping estimates (URS, 2000 describes the necessary number of wells for monitoring).

Review #4

CALFED BAY-DELTA PROGRAM

The reports concerning the CALFED Bay-Delta Program represent a significant effort to understand the dynamics of this complex, human-impacted, ecosystem. My review of the documents for this program has focused primarily on the areas of organic matter dynamics and their potential impacts on drinking water resources. I attempted to understand how estimates of dissolved organic carbon (DOC) and total trihalomethane (TTHM) concentrations were derived, and how this information was applied to questions of water quality impacts. I did look through all of the material, but put most of my efforts into three documents: Draft Report on Water Quality Investigations; Water Quality Modeling Technical Appendix; and Synthesis of Data for Development of Reservoir Island Organic Carbon Model. My overall impression of these documents is that the Department of Water Resources has done a commendable job with the available information, but that the information base is too limited to provide a satisfactory prediction of the impacts of the proposed In-Delta Storage Program on water quality. I found a high degree of uncertainty contained in these documents, some of which is articulated, a dependence on numerous assumptions, many of which have not been adequately scrutinized, and several important questions that have not been addressed. Monthly averaging of historic data has the tendency to moderate predictions of high concentrations that occur over what are probably operationally important time scales for drinking water utilities. From the available data, I suspect that the required controls of DOC and TTHM concentrations will not be achieved.

Two areas of great uncertainty that are recognized in these documents by the consultants who prepared them, are the generation of DOC from the flooding of peat soils and the contribution of DOC and particulate organic carbon (POC) from the photosynthetic production of algae and rooted aquatic plants. Other areas of uncertainty that are not adequately addresses include the relationships between TTHM formation potential and organic matter quality, the biodegradability of peat-derived organic matter, the impacts of annual variations in wet and dry years, the appropriate time-scale for model estimates when considering drinking water quality, the relationship of historic DOC measurements to TOC concentrations, and the role of photooxidation in water quality.

The estimates of peat-derived DOC are based on experiments performed in the SMARTS facility involving large tanks where the experimental treatments involved soils of differing organic content and variable water depths. Data from these experiments were fit to a logistics equation that predicts DOC concentrations from flooded peat soils as a function of storage time, the starting DOC concentration, and a growth rate for DOC production. The methods involved in the experimental manipulations of peat soils within the SMARTS facility are not clearly described. For example, how were the experimental chambers filled with peat soils and how were these soils collected? I suspect that significant disturbance of the soils and their structural integrity occurred, and once disturbed, I think it would be difficult for the soils to be representative of field conditions. It is not surprising that there is an initial, rapid increase in DOC in the overlying water following the setup of the SMARTS tanks, and I further think that most of the DOC generation was through the abiotic leaching of soil organic matter rather than microbially-induced generation of DOC through the degradation of POC. The relevance of the Hopkinson et al. (1998) investigations on estuarine planktonic bacteria to the peat soil microbiology is questionable. It would be more useful to look at work on the Kuparuk River that drains peaty soils and delivers organic matter to Toolik Lake in the Arctic LTER or studies involving CO2 emissions from tundra soils under models of global warming to get a more realistic idea of the microbiological activities that could be anticipated. Ideally, studies of biodegradability would be performed with extracts of the Delta peat deposits. Additionally, more realistic studies involving undisturbed peat cores, either by using smaller samples in a laboratory setting, or by placing mesocosm structures over soils directly in the field, should be considered.

I am not familiar with the standard agricultural soil saturated paste method cited in the MWQI-CR#4, but those data are probably only useful in establishing a upper range for potential releases of organic matter following disturbance. Similarly, I am not familiar with the issues surrounding tilling the peat soils, but suspect that such an activity could lead to an initial large release of organic matter followed by land subsidence and soil compaction. Another area not considered in evaluating the DOC dynamics of peat soils is the potential impacts of photoxidation on organic matter in the Delta waters. Photooxidation of DOC is not a simple issue, but a summary of that field of inquiry seems to be that photooxidation has the impact of making organic matter that is refractory to biological degradation (such as humic substances) more biodegradable, and making biologically labile constituents (such as algal exudates) less biodegradable.

Background data for the logistics model that includes values for starting DOC concentrations come from monitoring data for DOC and UV absorbance in the Delta region and a parameter derived from these values, the specific UV absorbance. After plotting the existing data for these parameters, there is an attempt to compare visually the relationship of DOC and UV absorbance by fitting a polynomial curve to the data. That approach is of questionable validity as there is no theoretical basis for the shape of these curves and a mathematical examination of the correlation between variables is more informative than “visualization”.

The Twitchell Island data for DOC and UVA254nm for lysimeters and piezometers provides valuable information on temporal and spatial variation in concentrations as well as some insight into potential impacts of agricultural practices. The data in Table 3.3-2 on p. 55 of MWQI-CR#4 for piezometer 7 show some of the temporal variability in the relationship of DOC to UVA, and should be a cautionary note for carrying those relationships, based on long-term averages, too far. UVA is influenced by water quality, especially the presence of carbon-to-carbon double bonds and color involved in the adsorption of light, whereas DOC values only reflect quantitative differences. Despite high correlation coefficients for the relationship of DOC to UVA, such as that shown for Greens Landing in Water Quality Modeling Technical Appendix, disparities exceeding 1 to 2 mgC/L are readily seen in the data (Figure 9). Changes in DOC quality that would impact UVA values are typically associated with changes in hydrology and flow paths that are often the times when prediction is needed, but also when the accuracy of the predictions is least robust.

The approach to estimating DOC concentrations based on response to flow is an interesting and potentially powerful step towards understanding the dynamics of the Delta system. However, large disparities between observed and generated DOC (see Fig. 6 in the Modeling Appendix) show that the dynamics of the system are not being captured in many cases, at both high and low concentrations. Monthly averages of observed and predicted concentrations are much tighter, but this approach tends to obscure the concentration extremes and misses important temporal dynamics that drinking water utility operators will be forced to respond to, both in terms of TOC concentrations and TTHM formation potentials.

The application of predictive equations to derive TTHM constraints is another area where I think inadequate information is available to determine the impacts of the Delta project. This is outside my area of expertise, but I believe that generalized equations, such as that obtained from Malcolm Pirnie and presented in Attachment 2, are usually based on analysis of water from a single source or watershed. Because both TOC (or DOC) and UVA are variables in the equation, and these change with sources, there is a need to validate the predictions with data from the Delta system. TTHM formation potential test could be run with Delta waters as a check on the assumptions made in the model development. One of the assumptions, that DOC = raw water TOC, is certainly not valid. Unless the approach advocated by the USGS for the measurement of suspended (SOC) or POC is used, wherein a separate sample is analyzed following collection of POC on a filter, the concentration estimates of TOC will underestimate the true values. There are not only issues of efficacy of oxidation, typically addressed by using high temperature combustion to oxidize particles, but also issues of collecting a representative sample from a suspension and then getting particles into the instrument oxidation zone, not typically considered in TOC analyses. These concerns will be exacerbated by the presence of suspended sediments and turbidity that result from flooding of the Delta islands.

The concept of applying fingerprinting simulations to the Delta storage investigations are limited in the Technical Appendix to water flows. An expansion of this concept to involve the actual fingerprinting of organic matter sources is a relatively new area of scientific investigation, but one that could be valuable to understanding the Delta ecosystem. Consideration of the specific UVA is a step towards this process, but inclusion of molecular-level tracers, either through analyses of individual molecule species or analyses of bulk properties such as fluorescence, and then application of those data in mixing models, might help decompose organic matter in the Delta into the contributing sources.

Review #5

Science Panel Review of the CALFED In-Delta Storage Program's Reports on the Delta Wetlands Project: Scientific and Technical Review

General:

In the following discussion, I will use the following abbreviations:

SUM = Draft Summary Report
ENV = Draft Report on Environmental Evaluations
MOD = Water Quality Modeling Technical Appendix
DOC = Synthesis of Data for Development of Reservoir Island Organic Carbon Model in DSM2 Model
OPS = Draft Report on Operation Studies
WQ = Draft Report on Water Quality Investigations
DWP = Delta Wetlands Project (general, any incarnation or alternative)
Bay = San Francisco Bay, downstream of confluence of Sacramento & San Joaquin rivers

I have read the vast majority of the material provided for this review and have many, many comments and questions, which I list below. Here I attempt to summarize my more detailed comments and questions:

1) In general, uncertainties (e.g. estimation errors) were not quantified for the water quality and environmental estimates provided (i.e. for model-based estimates, etc). I believe that, for a project of this magnitude (Order [$1 billion]), decision makers should know how large the uncertainties are. Many of the comparisons between base (no DWP) and DWP cases showed only very small differences in the quantities compared (water supply, water quality, etc.); because the differences appear quite small, attention to error and uncertainty is especially important (i.e. whether the benefits of the project are smaller or larger than the size of the estimation error). In addition, in many cases, details of an approach and assumptions made in calculations were not revealed or made clear. Assumptions and approaches are, in my opinion, at least as important as the results: in some cases, I could not evaluate results because I was not given ample information to understand the approach.

2) As I detail below, I have several major reservations with respect to the approach used for modeling DOC. Some of these issues could result in an overestimate of DOC released from reservoir islands, others could result in an underestimate, and others could result in either. Therefore, I am not able to verify the reasonableness of the DOC results. Further, since estimates for UVA, TTHM, and Bromate are based on estimates of DOC, I can not verify the reasonableness of those results either. I suggest a totally new *process-based* approach (as opposed to *curve-fitting*) be developed for dealing with the DOC term.

3) I believe that vertical temperature stratification of the reservoir islands (while filled) is a distinct possibility, since 1) they will not be subject to tidally induced mixing and turbulence; 2)

they will be relatively deep; 3) summer air temperatures in the Delta can be high; 4) colleagues have recently seen evidence of at least temporary stratification in other Delta environments near the proposed reservoirs though not as deep and subject to weak tidal mixing. In many of the studies for this project, the water column was assumed to be vertically well-mixed due to wind mixing. I do not believe this is a safe assumption. Further, I believe stratification will bear directly on the water quality (TOC, temperature, dissolved oxygen, and other constituents) within and released from the reservoirs (this point was acknowledged in ENV and in the biological productivity assessment included in WQ). I suggest much more thorough investigation of vertical variability within the proposed reservoirs be performed, and that this may be done best with a multidimensional numerical model. Additionally, even if the basins are characterized by uniform bathymetry, horizontal variability in physical and biogeochemical processes may---and probably will---develop due to biases in wind direction and development of secondary flows in corners and coves. As I understand it, DSM2 cannot account for vertical or horizontal variability within reservoirs; therefore, for the purposes of understanding in more detail the internal and adjacent dynamics of the proposed reservoirs, I suggest another (three-dimensional) model be employed. The first stage of such a modeling effort would be purely hydrodynamic, to understand interactions between wind, geometry, surface heating, inflows, and outflows. Later stages would include incorporation of other important water quality constituents of interest (e.g. TOC, DOC, dissolved oxygen, TTHM, Bromate, UVA, possibly submerged aquatic vegetation, etc.) to understand three-dimensional spatial and temporal variability of these constituents relative to timing and location of reservoir discharges. Results of such detailed studies should yield important information regarding optimal placing of diversion and discharge points. Multidimensional model results could be compared to the more efficient one-dimensional DSM2 results to generate error bars on the DSM2 estimates or to refine the representation of key processes within DSM2.

4) Although I believe the several separate studies reviewed here constitute, in general, a good "start" toward investigating the feasibility of the DWP, I suggest a more integrated approach be taken in the future. For example, water temperature and dissolved oxygen were studied separately from DOC, TTHM, and bromate dynamics. Because 1) DOC transformations are supposedly sensitive to temperature, 2) TTHM and bromate depend directly on DOC, 3) other biochemical processes such as algal and macroalgal growth relate to most of the above constituents, I suggest these all be modeled and studied in tandem, with the same sets of hydrological, physical, and operational assumptions in place for all constituents (many of the studies to date were performed with different sets of assumptions and constraints) and with feedbacks between constituents explicitly incorporated and permitted. Separate study of constituents may preclude an understanding of potentially important feedbacks and relationships.

<u>Specific Comments and Questions</u>

<u>*SUM:*</u>

1) I realize the incorporation of X2 into the operation guidelines represents an attempt to consider the general effects of the DWP on the downstream estuarine system. Are there any studies planned for evaluating more detailed effects on Bay physical and biological processes of

holding back freshwater (and possibly sediment and other transported constituents) in the DWP? I recall previous two-dimensional modeling work on Suisun Marsh suggesting changes there could cause water quality effects to propagate *upstream* into the Delta. I certainly believe it possible for analogous effects to propagate *downstream* to the Bay from massive changes in the plumbing and possibly water quality of the Delta.

2) Is it still expected that construction on the DWP would begin by end 2002? (This does not seem feasible to me). [SUM 2]

3) I am curious why there was no discussion of the possibility of restoring the habitat islands to tidal action, as opposed to controlled siphoning of water into and out of those islands [SUM 53]. I realize the habitat islands are meant to mitigate effects of reservoir islands on many interests (agriculture, sensitive species, etc.) and so those islands will not be entirely “wet.” However, I wonder whether it would be more consistent with CALFED’s ecosystem restoration goals to render portions (e.g. intended wetlands) subject to the more natural forces of tides. (I am not a wetlands ecologist and so this is purely a question for such experts.)

4) Have the effects of perimeter seepage pumps on adjacent channel hydrodynamics been considered? [SUM 12]

5) Have the levees bordering islands adjacent to the reservoir islands been evaluated for stability vis a vis the hydrodynamic forces of discharge? What about stability of these nearby levees in the case of reservoir levee failure?

6) 2020 level of demand and hydrology was assumed for the project. Since this is supposedly a project with a 50-year lifetime, why not explore either 1) the worst-case (e.g. 2055), or 2) a reasonable stepped progression from project start (e.g. 2005) through its lifetime (e.g. 2055)? If we assume the simplest (linear) relationship between time and demand, a 2020 level would not even represent the average conditions for the project lifetime. [SUM 21]

7) Have any quantitative studies been done to predict the reservoir discharge flow partitioning between various destinations (e.g. pumps, Bay) under different hydrological and operational scenarios? I did not see anything quantitative in this respect, and suggest a “finger-printing” exercise like that discussed in MOD, but with numerical tracer sources associated specifically with the 4 DWP islands. Such an analysis should incorporate variability due to operations of Delta Cross Channel, gates, barriers, pumping, and seasonal and interannual hydrology, which have been shown to have Delta-scale effects. These analyses would allow interested folks to know *how much* of reservoir or habitat island discharges travels to urban intake points.

8) I am clearly not an economist, but could not *not* comment on the following observation: total annual pumping demand (state+federal) is 7-8 MAF; estimated annual yield from storage islands is 100-200 TAF; under the best case, then, the proposed project would provide approximately an additional 3% worth of annual pumping demand. How does that 3% estimate compare to the magnitude of the uncertainty in making the calculation? (i.e. is it possible the uncertainty is greater than 3%, in which case the DWP could conceivably yield zero or a negative net gain?) Has anyone quantified how much more water we (will) need beyond that which we (will) get

without the DWP and whether the 3% gain will help substantially, and will it be worth ca. $1billion? Further, it was stated that delta smelt constraints could reduce the project yield by 10-20%. DOC constraints were projected to have similar effects. My impression was that these two potentially large impacts were not simultaneously incorporated into projections of project yield and cost per TAF, but I suggest that they are. I realize this is the job of the economists, and that they have attempted to compare the worth of the water provided to the cost, but it is not clear to me whether they have arrived at a projected net cost of the provided water based on all possible impacts and whether the gain will be worth the investment. My impression is that this work is still in progress.

9) [SUM 31 and elsewhere] I do not understand the meaning of “reoperation.”

10) A (possibly stupid) question: If we are interested in more, safe storage, why not dig out the reservoir islands and use the dirt to bring new habitat islands (whether DWP or non-DWP) to intertidal levels? I realize there are probably lots of complicating issues like cost of moving dirt, mercury exposure, etc., but I wonder if this was ever considered.

11) It was stated that “wave run up and set up on the reservoir” was accounted for in the engineering design [SUM 35]. Does “wave set up” mean the same as “piling up” of water on one end of the reservoir due to sustained wind drag at the surface? I suppose that it probably does; however, if not, then the “piling up” effect should be considered.

12) Has operational noise (due to pumps, etc.) and its possible effects on wildlife and recreational use of the islands been considered?

13) [SUM 49] It appears that a 100-year flood was used in the most recent engineering design, but that a 300-year flood may become the controlling design criterion. With all the uncertainty involving climate change in the next several decades, I believe it is best to design based on the more stringent criterion. Is the 100-year (or 300-year) flood “upgraded” for effects of climate change?

14) [SUM 49] Do the costs outlined in Table 10 include the costs of habitat island purchase and some degree of development?

15) [SUM 52] What is meant by “changes in flow patterns?” Do the authors mean “spatial” or “temporal distribution of flows? What changes are expected, and what are these expectations based on? Also, what is meant by “reductions in transport flows?” Have these things been modeled? Need they be, for the purpose of projecting flow-related impacts on sensitive fish species? Recent studies at the Delta Cross Channel have suggested that young migrating fish do in fact “go with the flow.” Therefore, it may be useful for hydrodynamic modelers of the physical effects of the proposed DWP work closely with fish biologists so that the biological impacts of specific changes in flow may be considered. Multidimensional modeling may be necessary here, as well.

16) [SUM 57] It appears that mitigation costs associated with the habitat islands (initial cost ~$600 million, annual cost ~$7 million, Tables 13-14) are *in addition* to the storage related costs

detailed in Table 10 (?). Are these mitigation costs going to be incorporated into projections of the costs for reservoir project yield (i.e. $/TAF)?

17) Table 17 [SUM 61] presents costs per TAF for water yielded by the DWP, without discounting project yield for delta smelt and DOC constraints. Therefore, these costs represent the "low end" or "best case." In the benefits column, if recreational benefits are included [SUM 62], then should the costs of providing those benefits also be included in the project cost? It will be interesting to see the final project of cost per TAF once all constraints and costs are incorporated. At this point, it is difficult for me to discern whether, dollar-for-dollar, the project benefits will be worth the costs (I did not see a "bottom line" arrived at in the economic analysis).

<u>*OPS:*</u>

1) Were extreme climate cases, such as El Nino, La Nino, and extended drought events, considered as part of the hydrologic variability incorporated into the simulations?

2) Has the effect of diversion to storage on X2 been modeled or projected in some quantitative manner? What are the maximum diversion rates based on?

3) There seems to be a contradiction in times and allowances for Webb and Bacon [OPS 7-8].

4) It was suggested that the reservoir islands could serve as fish rearing or spawning habitat when flooded to shallow depths. How well will this work when the islands are essentially "closed" to at-will fish immigration and emigration? [OPS 9]

5) It was also stated that "water will be circulated till deep water flooding occurs" ---HOW? [OPS 9]

6) With respect to limits on "export of stored water" and "discharges for export," how does anyone currently know how much of the stored water will be exported (presumably by the pumps in the southern Delta)? This hearkens back to my suggestion under *SUM #7* above. [OPS 10]

7) DSM2 model-generated salinities were correlated with flow. Why weren't measurements used instead? (Perhaps measurements do not exists where the data is needed?) More importantly, how well does DSM2 do at EC prediction (i.e. what is the uncertainty or error involved in these estimates)? [OPS 12]

8) I did not understand the methodology for CVPIA and EWA operations. [OPS 13]

9) The purpose of the CALSIM model "is not to recreate historic conditions but to predict potential conditions under various system, regulatory and water demand scenarios." I understand this, but how well can the model do in recreating historic observed conditions? (i.e. Can we please see some validation data, please?) What is the error associated with this model? [OPS 15]

10) I do not understand the methodology described for maximizing storage project yield [OPS 15].

11) How are flow-allocation decisions made at nodes in the CALSIM model? This is a very important aspect of the model which could easily affect outcomes. [OPS 17]

12) If 10% of project yield becomes EWA water, then is that a further reduction in project export yield (i.e. in addition to reductions for delta smelt and DOC)? [OPS 18]

13) Why is the minimum required delta outflow different for base and storage cases? What should the delta-scale water mass balance be? (I do not understand all the terms in Table 4, and so could not say whether this made sense.) [OPS 19, Table 4]

14) It looks like there is essentially no improvement for low delivery periods. Does this mean that the DWP offers no improvement in water supply during cases of drought? [OPS 21, Fig. 3]

15) I do not follow the discussion regarding carry-over water, "risk in allocating deliveries," etc. and so cannot say whether this makes sense. [OPS 23]

16) Is it possible that DOC constraints on reservoir discharge, coupled with internal DOC increases over time, could cause DOC concentrations to rise to/past a value which would forever prevent discharge? If so, what do you do? [OPS 23]

17) It is stated that "climate change creates more surplus water in the delta for in-Delta storage to capture" [OPS 24]. Based on the values in Table 7, I would say this is *barely* the case. Again, is this estimate of extra surplus water larger than the error bars on the model used to do this calculation?

18) My understanding is that the effects of climate change, delta smelt constraints, and water quality constraints were not considered simultaneously [OPS 24]. I suggest in future evaluative studies that simultaneous consideration of all major constraints is attempted. (I know—it'll be hard.)

DOC:

1) I do not understand what the values in Tables 3.2-1 and 3.2-2 are "recommended" *for*. For use as initial conditions for the DSM2 model? (I thought they did a "cold start.") Values in Table 3.2-1 are based on an initial shallow fill depth of <2 feet---how is depth taken into account in getting these values? Are the annual averages in Table 3.2-2 set within the model? (If so, then does that mean the outcome is constrained artificially from the start?) [DOC 46]

2) Where was the soil in the SMARTS experiments from? Is it representative of what we would expect at Webb and Bacon? If not from Webb and Bacon, why not?

3) I appreciate the great amount of effort that was expended toward developing an understanding of how peat soils contribute to DOC in flooded islands, and also appreciate how complex an issue this is. I also do believe that the idea of a peat-soil-focused experiment was a good one. However, I have several questions and reservations about the scientific soundness of the SMARTS experiments and, therefore, the scientific soundness of the DOC model based on the experimental results. For example:

a) It is claimed that one significant difference in results between tanks was due to the difference in the peats used (i.e. some had "high" OC content, others had "low" OC) [DOC 48]. It was stated that "a rainstorm had leached and drained away much of the soil organic matter" [DOC 62]. Was this an accident? There was no mention of having actually a priori measured the OC content of the experimental soils so that the relative OC content of the "high" and "low" OC soils could be quantitatively compared and the effects on measured DOC in the tanks soundly inferred. I realize that the peat soil water DOC concentrations were measured during the experiment, and those indicated possibly "higher" and "lower" soil OC content, but those measurements were performed in the context of other varying experimental parameters. Was the difference in soil OC contents inferred from the experiment results or actually directly measured a priori?

Further, the tanks containing the "high" OC soils contained water at a depth of 2 feet, while the tanks containing the "low" OC soils contained water at a depth of 7 feet [DOC 48, Table 3.3-1]. Therefore, the effect of differences in OC content of the soils cannot be separated from differences in water depth. These two factors (soil OC content and water depth) are effectively lumped together in comparisons of observed DOC concentrations between tanks [DOC 62]. These two factors should not be lumped together, since 1) the mechanisms governing DOC concentrations associated with the 2 factors are distinct, and 2) the effects of each factor could be large. If the intention had been to test the effect of different soil OC contents, then that factor should have been varied against constant water depth, peat soil depth, etc. In essence, it appears that there were actually 4 independent variables involved in the experiment (peat soil OC content, water depth, peat soil depth, water exchange) but two of those factors (incidentally, the two bearing most on the DOC model development) were lumped together and not tested independently of each other.

b) Were the tanks mixed at all vertically? No mention was made of intentional or unintentional mixing of the tanks, and temperature differences between pore water and surface water (in summer, ~3-4 degrees, Figs. 3.3-1, 3.3-2) suggest that they were probably not mixed. Also, the Water Temperature and Dissolved Oxygen study (WQ 156) mentioned that the SMARTS tanks had been covered, which reduces the chance of wind-induced mixing. If the tanks were in fact not mixed vertically, then the DOC content in the surface water (where samples were taken) could easily have been limited by the rate of vertical molecular diffusion of DOC up from the soil to the water surface. Using molecular diffusivities for heat and salt as bounds for the possible diffusivity for DOC, I have estimated that it could take anywhere from 1 month to 11 years for DOC to molecularly diffuse from the soil to the top of the 2' tank and from 1-120 years for DOC to molecularly diffuse from the soil to the top of the 7' tank. Chances are that unintended mixing did occur due to night time surface cooling and convection or due to accidental mechanical stirring while sampling. However, these estimates suggest that it could easily take months for DOC to diffuse from the soils up to where DOC measurements in the tanks were made.

Therefore, I ask: Are the surface DOC measurements from the tanks representative of the DOC concentrations in the proposed reservoirs? I doubt it because the reservoirs will at least be subject to wind-induced turbulent mixing (i.e. even if the reservoirs stratify, I bet it is unlikely that they would see top-bottom differences of 4 degrees). Therefore, I expect the SMARTS-based estimates of surface water DOC concentrations to be underestimates of surface DOC in the reservoirs.

Another related question: Do we know from what elevation reservoir water will be discharged? If not from the reservoir surface, then the SMARTS experiment could further underestimate the DOC discharged from the reservoirs.

Also, the transformation rates of OC are said to be sensitive to temperature; therefore, effects of stratification could affect not only vertical transport and distribution of DOC, but also DOC "production." [DOC 48]

c) DOC concentrations appear only to have been measured in the surface water and peat soil pore water within the tanks. Were vertical profiles of DOC within the tanks measured? If not, why? Such profiles could help separate out processes governing DOC in the water such as vertical diffusion and production in the water column, as well as assess how representative surface measurements are of the entire water column.

Furthermore, a DOC model based on surface water samples from an incompletely mixed tank reactor may not be appropriate for use in a depth-averaged model such as DSM2.

d) No mention was made of whether other sources of DOC (e.g. algae) may have been present.

e) How do the temperature and light conditions potentially governing OC transformations in the tanks compare to the field?

f) [DOC 49-50, Figs. 3.3] When water depth (H) = 2 ft, why would peat water be higher in DOC for peat layer depth (PL) = 1.5 ft than for PL = 4 ft? Why were there differences in temperature (T) for the peat water for H = 2?

When H = 7 ft, why was surface water DOC higher for PL = 1.5 ft than for PL = 4 ft? This is the opposite of the trend for H = 2 ft. Why are T's different for same H but different PL?

These trends were not discussed, and impact of peat layer was not interpreted or incorporated into the model, although it was supposedly one of the independent variables which the experiment was designed to study.

g) *Where* in the peat layer (i.e. at what depth) were the peat water measurements taken? Measurements of DOC in wetland soils from Lake Apopka, Florida, demonstrate that soil DOC gradients can be very sharp; therefore, the depth at which DOC measured in the soil is important [DOC 59].

h) With respect to Figs. 3.3-1, 3.3-2 [DOC 49-50], the report suggests that the "rate of DOC production" slows around day 150, at which time peat soil DOC concentration begins to decline [DOC 51]. To my eye, the rate of DOC production in most cases appears to slow around day 250, i.e. I believe that there is a time lag between DOC soil peak and decline in rate of DOC appearance in surface water. Further, I propose that part of this time lag may be regulated by the vertical diffusion discussed above in *DOC 3b*.

Also, the term "production" implies that DOC is being produced via in situ transformations within the surface layer. Do we know that is in fact the case? i.e. how do we know it is not merely transport up from the bottom layer, in which case the appropriate term would be "appearance" or "accumulation?" Because the approach taken in conducting these experiments was not highly process-based, it is difficult to separate out which processes are responsible for appearance and disappearance rates of DOC.

i) Suddenly the report started referring to "TOC/DOC" as opposed to "DOC" [DOC 51], but I don't know what "TOC/DOC" means. Is the author referring to a ratio? Or does he suggest that one is interchangeable with the other? I don't believe any specific mention was made of measuring TOC concentrations, nor were any shown. On the other hand, the separate Biological Productivity study [WQ 123] reported TOC-DOC differences in the SMARTS experiments of about 10 mg/L (not mentioned in the DOC report). If that TOC-DOC difference is accurate, then TOC and DOC should not be taken as interchangeable.

4) Below are questions and issues I have with respect to the actual DOC algorithm developed based on the SMARTS experiments.

a) I am not comfortable with the form of the DOC "logistics equation" which requires specification of a *maximum* DOC concentration (parameter "A"). There are several reasons I do not think this is a good idea:

i) Pre-specification of such a maximum may artificially constrain the model results.

ii) Constraint of maximum predicted DOC concentrations for a flooded island is based on experiments which may or may not well represent conditions on the flooded island (e.g., see discussion above in *DOC 3b*); therefore, the maximum DOC based on experiments may not be appropriate for the field.

iii) There are two cases developed (each with their own max. DOC) based on the "high OC soil/small water depth" (the "high bookend" case) and "low OC soil/large water depth" (the "low bookend" case) tank experiments. As discussed above (*DOC 2*), we do not know how representative the soils were of the flooded island sites, nor how different the actual soil OC contents were. Further, it was stated that the "A" term accounts for differences in soil OC content *and* differences in water depth [DOC 62]. But, as discussed above (*DOC 3a*), we do not know how much of the effect on "A" is due to the soil OC content and how much is a reflection of water depths, since these factors were not studied independently.

iv) The DOC prediction equation for each bookend case was based on the *average* of two tank experiments. Therefore, the so-called "maximum" DOC allowed in the prediction algorithm is not even as high as the maximum seen in the experiments, which I suggested above may be lower than field conditions.

v) Model results are very sensitive to the maximum DOC concentration specified [DOC 68, Fig. 3.4-7], so users of the logistics equation need to be *right* when choosing a value or range of "A." However, there is no guidance offered on how best to choose "A" to get a reliable prediction for a flooded island.

b) Again, there was no mention of effects of peat soil depth and apparently no attempt to incorporate it into the DOC algorithm. Why? Is it not important?

c) An equation was presented [DOC 64] for calculating a DOC mass balance, accounting for dilution of DOC by the water depth and inflow of DOC and water from the river. I believe this equation is incorrect, in that it does not weight the river inflow term by a ratio of river inflow volume to final reservoir volume. The weights on the right side of the mass balance equation should add up to 1, but they add up to more than 1, meaning a DOC concentration calculated with this equation would be overestimated (assuming inputs to the equation are correct). It is not clear whether this equation was actually used in the DSM2 predictions of DOC variability; it is possible that it was not, since a correct mass balance equation was presented for EC [MOD 18], which may have (hopefully) been applied to DOC as well.

d) I have questions with respect to the DOC "growth rate," k, which is said to be sensitive to water temperature [DOC 64]. The full year of Experiment 2 data were curvefitted to get the "high" and "low" bookend equations, including estimates of "k." Although, the logistics equation was fitted to the full year, (not just the winter portion), the resulting "k" value is said to be representative of winter [DOC 65]. Was a curvefitting performed for just the winter subset of data to get this "k" and just never mentioned?

The Experiment 1 results, which did take place during summer/early fall, were taken to yield a higher "k" value representative of summer, but these results were not shown. There was at least one other major difference between Expt. 1 and Expt. 2 conditions: Expt. 1 included water exchange, while Expt. 2 did not. Can we then safely say that the winter and summer "k" values are comparable, since one was arrived at under different hydraulic conditions (the effects of which, incidentally, were not discussed)? If the increased exchange in Expt. 1 enhanced dilution, then the "summer k" value is probably an underestimate when compared to the non-exchange Expt. 2 "k" value.

Also, there appear to be two different "k" values for the summer (Expt. 1- based) case—one each for the high and low bookend cases [DOC 65, Table 3.4-2]. Should "k" (growth rate) depend on soil OC concentration? How were these values arrived at?

e) The high growth rate case shown in Fig. 3.4-6 [DOC 67], its oscillatory behavior, and explanation of "chaos behavior" puzzle me. Is there a numerical instability in this model? Is there an inherent instability in the algorithm or its application which could feed into the DSM2 simulations? This behavior does not look reasonable to me.

f) The logistics equation does not include separate terms for sources and sinks of DOC; rather, it attempts to directly relate (via curvefitting) the dependent variable (DOC in surface water) to the independent variable (time), and in the process lump underlying mechanisms together via specification of the equation parameters (A, B, k). I believe such a process-light curve-fitting approach is dangerous when several processes (e.g., production of DOC in the sediment and in the water column, flux at the sediment-water interface, vertical transport of DOC in the water column) may govern DOC concentrations in the real system and when those processes do not appear to have been separated and compared adequately to warrant such a simplified representation.

Since "k" supposedly depends on water temperature and therefore varies substantially seasonally, why not calculate DOC (or rate of change of DOC) dynamically via an equation which attempts to separately account for the major processes? Such an equation could look like:

$$DOC_{water\ column} \text{ or } \frac{dDOC}{dt}_{water\ column} = \textit{sed-water flux contribution}$$

+ water column vertical diffusion and mixing contribution

+ water column production or transformation contribution (function of k)

+ horizontal exchanges or flushing contribution

where "k" would vary monthly or at least seasonally due to temperature effects (including the non-monotonic relationship with temperature for high temperatures [DOC 48]), as might vertical diffusion, flux, and mixing rates. This type of approach could be developed to more naturally replicate the annual cycle based on a combination of processes, as opposed to a highly constrained curve-fitting approach. Admittedly, such an approach takes work to develop reasonable representations of the processes, but the benefits would be great. I recommend such an approach so that: 1) explicit accounting of processes is done; 2) while representations of some processes may be drawn from the experiments, it is not implicitly assumed that the experimental tanks are in all ways equivalent to the field conditions; 3) the model has the "freedom" to respond to the combination of conditions present in the modeled system, as opposed to being constrained to respond in the (much simpler) way represented by strict non-mechanistic relationships we've prescribed; in other words, models should be able to "surprise us" and squash our own often simple expectations; 4) results can be dissected process-by-process, so we know how much of the predicted DOC concentration is due to, for example, flux from the sediments versus water column production.

g) I don't completely understand the seepage and seepage return processes expected on the islands, but I wonder if it is more reasonable to assume a higher return concentration based on the measured soil DOC from the tanks? (The 20 mg/L assumed in the estimates [DOC 69] is on the low end of soil DOC in the tanks.) Also, it is stated that the effect of the return flow would be "additive"—if it is intended that an approach similar to that in the equation on DOC 64 is to be used, then that approach would be incorrect for the reasons discussed above under *DOC 4c* (returns would have to be weighted volumetrically and then added).

h) I agree that contributions of plants (e.g. vascular, phytoplankton, epiphytes, benthic microalgae) to the DOC pool need to be incorporated into the approach for predicting DOC concentrations, as the current approach only accounts for soil-related contributions.

i) In the Modeling appendix [MOD Part F], it is stated that when the DOC logistics equation is implemented in DSM2, an inverse power law water depth adjustment is applied, as recommended by Jung (2001). No description of this was included in the DOC report, let alone justification and derivation. Why is this adjustment used? What was the basis of its development? The discussion of how it is implemented in the MOD report appears very "make-shift." If water depth is already accounted for in the specification of "A" *and* in some mass balance computation for filling the reservoir with river water (like for EC), is this inverse power approach yet a third adjustment for water height? Is the model diluting DOC thrice over? Moreover, can we apriori assume that DOC concentration will always automatically decrease if water depth increases? Can there not be mechanisms which develop in a deeper water column (e.g. stratification and algal growth) that could potentially enhance DOC levels? Again, this approach strikes me as dangerous "curve fitting" without representing underlying processes.

j) Also in the Modeling appendix [MOD Part F], it is stated that the DOC growth contribution is curtailed once the storage depth becomes smaller than 1.5 ft. Why?

5) Does the *kind* (e.g. the bioavailability) of DOC matter, with respect to the water quality considerations at hand? In other words, do we know whether all sources of DOC to the water column are equivalent mg-for-mg ?

<u>*MOD:*</u>

1) The outflow EC and DOC values for the habitat islands are set to annual averages of observed concentrations in the Delta. How representative do we expect those observations to be? How much intraannual variability is there in these values? If intraannual variability is substantial, then I suggest that at least seasonally variable outflow concentrations be used. [MOD 8]

2) I do not completely understand the DICU approach [MOD 9]. Is this saying that total consumptive use is assumed to not vary over the life of the project---just the distribution of the use? If so, how realistic is that?

3) I do not understand how daily EC values were derived for the San Joaquin River. I did not find a thorough explanation of how it was done or how well the model performed and so cannot evaluate whether this was reasonable. [MOD 11]

4) It is stated that only one DOC growth rate ($k=0.022\ d^{-1}$) was used for the DSM2 simulations. This value is the lower of the values recommended in the DOC report and supposedly only representative of winter. If only one constant k value was used, then DOC may have been underestimated. [MOD 11, Table 3.1] Also, the low bookend value of “A” used in DSM2 appears to be 70 mg/l, as opposed to the value of 20 mg/l recommended in the DOC report. Why the discrepancy? Jung showed that the DOC approach is very sensitive to the prescribed maximum DOC value.

5) For prescribing DOC boundary conditions, measurements were used for summer, but apparently a “flow-DOC” relationship was used for winter. What is this relationship and how well does it fit observations? Also, Sacramento River DOC values were used for the Yolo Bypass---is this a good idea? Colleagues (Ted Summer) have shown that floodplains function differently than the adjacent river and may be quite productive; therefore, I suspect DOC in Yolo may not be well approximated by DOC in the Sacramento River (question for DOC experts). [MOD 12]

6) I don’t understand how/why CALSIM “frequently diverted small amounts of water to the project islands to account for evaporation losses.” [MOD 13]

7) [MOD 15-17, Figs. 4.4-4.5] Maximum predicted volume of both reservoir islands appears to repeatedly “peg” at some value less than maximum capacity. Why? Is there some unintended limit or constraint present in the model? This aspect of the results makes me question the reliability of model-based estimates of project yield.

8) A minimum chloride concentration of 10 mg/l was assumed when EC concentrations were very low. Why is this a good assumption? Are there no standard relationships that capture the low range? [MOD 19]

9) Water quality predictions with DSM2 were produced in the form of monthly averaged values [MOD 19], while compliance monitoring apparently takes place over short timescales [MOD 69]. Therefore, violations could occur but might get temporally diluted in a long term average. It is stated that the monthly averaging “smoothes out peaks in the results” [MOD 69]. Why would smoothing be a goal? Are not the peaks potentially the most non-compliant (and therefore

potentially most relevant) periods? Are the "peaks" reliable predictions of maximum concentration, or are they manifestations of numerical instabilities? If it is the latter, then smoothing is not the answer to the problem; a modification to the numerical method or time step could address that. I strongly suggest future water quality predictions be calculated at the time scales of expected monitoring. This is also recommended in WQ 35.

10) What is the difference between the early and late 1980's that accounts for the large difference in chloride mass loadings between the two time periods? Is it a large difference in hydrologic or operational conditions?

11) The implementation of Jung's DOC approach in DSM2 is not completely transparent to me [MOD 31]. Again, it is not clear how depth was taken into account. Further, problems with the asymptotic logistic equation seem to have appeared when applied within DSM2, with maximum allowable concentration ("A") sometimes being lower than it should be when river inputs are taken into account. To deal with this situation, a non-reactive DOC simulation was run and those results used when "A" would be exceeded due to river inflows. This seems to me a very unnatural and "make-shift" approach which probably underestimates DOC. An alternative approach which calculated DOC more "naturally," based on the range of contributing processes (e.g. river input, internal transformation, sediment flux, etc.) would not have the problems associated with a curve-fitting approach that necessitates prescription of a maximum allowable concentration.

12) Despite the large amount of effort in developing the DOC model, I personally can not believe the results presented on pages MOD 32-45, due to my reservations about the approach listed above (see *DOC 3&4* above).

13) Because of the large uncertainties I believe are associated with the DOC approach, I can not believe the UVA calculations since UVA is modeled as a linear function of DOC. [MOD 46-50]

14) Because of the large uncertainties I believe are associated with the DOC approach, I can not believe the calculations of TTHM [MOD 52-58], which varies with DOC. Further, I did not see any discussion of assumptions, basis data, or goodness of fit for the TTHM relationship [MOD 51, Equation 8].

15) Although I had some questions above regarding particulars of the EC modeling approach and would prefer to see direct validation of DSM2 EC predictions against measurements before vouching for the quantitative accuracy of the model's predictions (I'm quite sure such comparative data exist), I do not doubt the overall validity of the DSM2 predictions of EC and therefore chloride and bromide (chloride and bromide are calculated linearly from EC via high-correlation relationships), at least within the constraints and assumptions of the DSM2 one-dimensional framework. I expect the approach for calculating these 3 conservative constituents to be probably relatively solid.

On the other hand, recent USGS measurements of water quality in Delta channels and flooded islands have shown substantial lateral variability. Also, important transport and dispersion mechanisms that exist in a 2D or 3D world do not exist in a 1D world. Therefore, I suggest some multidimensional (2D or 3D) modeling be done for comparing distributions of EC (and perhaps other constituents) inside DWP islands, in channels, and at urban intakes between one-dimensional and multi-dimensional representations. This would help establish error bars around DSM2-based estimates of concentrations.

16) Because of the large uncertainties I believe are associated with the DOC approach, I can not believe the calculations of bromate [MOD 59-65], which varies with DOC.

17) I really like the use of the DSM2 model for the "fingerprinting" studies described in the MOD report [MOD, Part C]. I do have some questions and comments on this. Based on the description of how the source fingerprinting simulations were set-up (zero initial concentration everywhere in the domain, boundary or input concentrations of a steady 10,000 units each), I do not understand why any location in the system should have a sum of concentrations equal to 10,000 at dynamic steady state. If there is a very high-residence time region receiving very little flushing from adjacent waters, then that region may not necessarily ever obtain a concentration sum of 10,000 since it started off at 0. Perhaps this is just a lack of understanding on my part as to what is meant by "dynamic steady state." On the other hand, if an *initial* concentration was set everywhere to 10,000 units, then always, under all conditions, the sum of concentrations anywhere should add up to 10,000.

I suggest that this source fingerprinting approach be applied toward estimating the "envelope of influence" of the proposed flooded islands. Nowhere in the provided materials did I see a quantitative estimate of how much reservoir or habitat water would end up at a certain location by a certain time. If unique numerical dye sources were released from the DWP islands within a simulation, then we could know quantitatively the water composition at urban intakes, for instance, under various hydrologic and operational scenarios.

WQ:

1) When compliance with the water quality restrictions [WQ 11-12] is tested, where and when exactly are measurements expected to occur? i.e. What time of day? Where in the water column? Recent USGS water quality studies in the Delta have shown large diel variability at some locations in some water quality constituents such as dissolved oxygen and temperature. Furthermore, if the reservoir islands were to stratify vertically, then large vertical variations in some constituents (e.g., temperature, DO) would be expected. Therefore, it is critical that time and (vertical) location of results be representative of time and location of expected monitoring.

2) DOC was assumed to behave conservatively in the channels [WQ 20]. I strongly suggest asking a DOC expert on the scientific review panel whether that is a good assumption.

3) DOC was "used as a surrogate for TOC" [WQ 20]. I strongly suggest asking a DOC expert on the scientific review panel whether this is a good assumption.

4) I do not understand the temperature modeling approach. Apparently temperature was not simulated [WQ 20], but timeseries were used at urban diversions. Because of the small amount of information given, I can not say whether this is a reasonable approach.

5) It is stated that "reservoir releases for the high bookend condition rarely coincided with periods of peak reservoir DOC" [WQ 28]. However, I do not see that this is the case: In Figs. 2-7 and 2-8, releases occurred, perhaps not during absolute peak DOC concentrations, but almost always during near-peak conditions.

6) I wonder why there have been no estimates made (or at least mentioned) with respect to suspended sediment dynamics in response to the DWP. I think it might be useful to have a study done which explores questions like: Do we expect the reservoir (and habitat) islands to be net erosional or depositional? Is there a chance that they would "fill up" with sediment to a point where their storage capacity (or habitat functionality) becomes greatly limited within the lifetime of the project?

7) In general, I believe error propagation analysis should be applied to the calculations of water constituents like EC, Bromide, Chloride, DOC, TTHM, and Bromate, so that the sizes of *estimate-constraint* and *error* can be compared and, ultimately, an evaluation of the accuracy of the model relative to the accuracy needs of the study can be inferred.

8) "A large percentage of project releases…were drawn directly through Old River to the urban diversions" [WQ 34]. How do we know this? DSM2 results showed effects at urban intakes of island discharges, but I do not recall seeing this influence quantified. Again, I believe source fingerprinting could be applied toward showing this quantitatively.

9) Because I believe vertical temperature stratification may be an issue in the reservoir islands, I suggest at least one-dimensional vertically resolved representations of the relevant biological and geochemical processes be used to understand interactions between physical processes like thermal stratification, water depth, wind driven turbulent mixing, and convective mixing and biogeochemical responses like algal bloom formation and effects on vertical distributions of dissolved oxygen, DOC, TTHM, etc.

10) Although constrained by a short amount of time for completion, the Biological Productivity Study [WQ 115-137] is in my opinion an excellent start for identifying the range of processes potentially impacting organic carbon forms, levels, and discharges at the DWP reservoir islands. There was very little that I questioned or disagreed with in this study. Below are assorted questions and comments:

> a) It was assumed in this study that inflow to reservoirs occurred steadily over a 3 month period [WQ 117]. This appears to be different from other "quicker filling" scenarios explored in the DSM2 simulations.
>
> > i) If filling were to occur steadily over 3 months, what sort of current might be set up within the reservoir?

ii) I realize all parties on the overall DWP study were working with an abundance of preliminary data and “guesses” as to how the project might be operated, but optimally future studies would all be based on the same operational, geometric, and hydrological assumptions.

b) The authors mention the potential import of invasive clams [WQ 122]. The freshwater clam *Corbicula fluminea* has been shown to dominate phytoplankton dynamics in other Delta flooded islands and should be factored into future conceptual and numerical models for projecting algal sources of DOC.

c) 10-30 mg chl/m^3 for the Delta channels seems high in my opinion. [WQ 123]

d) How do we know “turbidity will decrease when channel water is put on the islands?” [WQ 123] This may not be the case if an energetic, erosional channel flows into a calm, stratified reservoir.

e) I appreciate the attention to rooted macrophytes and agree that they would probably lead to a more depositional system [WQ 125]. The effects of vegetation could also extend to temperature by causing more still conditions in which temperature gradients are not easily mixed out. This could have implications for transformation rates of OC and other constituents.

f) I do not necessarily expect the islands to be “well-mixed by the wind” [WQ 129], especially when full. The authors’ diagram of susceptibility to sediment resuspension (Fig. 4-1, WQ 119) suggests that wind-waves may not be adequate to reach the bottom and fully mix the reservoir under full conditions.

g) The authors estimate that microbial consumption can reduce DOC concentrations on time scales of days. They point out that the relative time scales for microbial transformation and channel transport to urban intakes are critical, since the DSM2 model currently does not include reactions for DOC in the channels. If the channel travel time is on the order of or longer than the transformation time, then the DSM2 model should include reactions in the channels (i.e. without reactions in the channels, the model could overpredict DOC concentrations at urban intakes). If the channel travel time is much shorter than the transformation time, then perhaps neglect of channel transformations is acceptable.

h) I strongly agree with the authors that a larger commitment of resources is necessary for modeling and experiments to be conducted in support of the ultimate ability to predict OC concentrations (or at least the reasonable range of possible scenarios) with confidence. I support a detailed, quantitative OC budget for the reservoir islands under different scenarios and operational assumptions including components related to peat OC flux, rooted macrophytes, epiphytes, benthic microalgae, and phytoplankton, as layed out in this Biological Productivity study.

11) Comments on dissolved oxygen (DO) and water temperature (T) modeling:

a) The approach taken in predicting DO and T provide a reasonable start. However, as acknowledged by the author [WQ 148, 152], some basic assumptions of the modeling framework employed in this study may limit the realism of the results. The use of a daily time step precludes resolution of potentially critical diel processes governing the variability of DO and T, such as formation and breakdown of diurnal thermal stratification, night time convection, solar heating, diurnal swings in photosynthesis and oxygen production and nighttime respiration losses. Recent USGS measurements in Delta flooded islands demonstrate strong diel patterns in T and DO, and recent finely temporally resolved simulations of water quality have shown that in some cases short term fluctuations of water quality can govern long term trends (i.e. they might not just get "averaged out"). In the flooded reservoir islands, because of the lack of tidally driven hydrodynamic processes, I would expect the diel cycle to represent a dominant time scale of variability. Therefore, as the author suggests, future predictions of DO and T (and possibly other constituents) should be done with a shorter time step (Order[hour]).

I also believe the possibility of thermal stratification should be acknowledged in future modeling exercises for this project, especially with respect to predictions of T and DO. The modeling work done thus far assumes that wind mixing will keep the reservoir fully mixed; however, when filled to capacity during warm months, I believe it is likely to stratify at least on the diurnal timescale and possibly persistently for several days. Stratification could set in motion several important processes such intense algal bloom formation (due to stabilization of the water column and isolation of algae at the top of the water column where light is abundant and benthic grazing effects are minimized), mass settling of phytoplankton biomass (due to attenuated mixing), bacterial decomposition of dead algae at the bottom, and the development of hypoxia or anoxia in bottom waters due to decay processes and inhibited entrainment of DO into the water column. It is stated [WQ 141] that modeling a stratified reservoir is not practical unless T profile data are available for model calibration and verification. I agree; however, I do believe 1-, 2-, and 3-dimensional verified models accounting for stratification are available which could do a good job of estimating what sort of stratification scenarios might occur in the DWP islands.

b) The heat budget equations employed are probably standard and fully reasonable [WQ 144-146]; however, the fact that they were not referenced, coupled with my inexperience with heat budgets, I was unable to verify their appropriateness in detail.

c) No description of the mass balance approach (for scenarios of island discharge) was given [WQ 146]. My guess as to what the equation would look like yielded estimates of river T similar to that shown in Fig. 5-22; however, the equation should be shown.

d) Again, no fault of this author, but different filling/draining scenarios (e.g. rates, times, capacities) were assumed relative to the other studies.

e) [WQ 147] Webb T predictions were compared with measurements from the Stockton Ship Canal. Were these measurements from the top, bottom, or mid-water column? (The SSC is known to stratify.)

f) Although availability of data limits comparison of predictions in some cases to sampling sites different from the expected discharge points, the author did the best he/she could do with what was available and the comparisons performed were probably not bad. Ideally, however, future more refined investigations should be supported by data at the actual discharge locations.

g) What would happen if the reservoirs stratify? Where would discharge water be taken from (e.g. bottom, top, or middle)? The bottom could violate oxygen constraints and the top could violate T constraints. Are there expectations of *adjustable* discharge elevations?

h) In addition to vertical variability of T and DO within the reservoirs, vertical variability of those constituents within the channels at the outflow points would be useful, i.e. does the channel water outside the reservoirs stratify during discharge scenarios?

Moreover, lateral channel variability at the reservoir-channel junctions is important for dilution of potential harmful aspects of island discharge water. I assume that the current mass balance approach (not shown) assumes full cross-sectional mixing, as does the DSM2 model. Such an approach may overestimate mixing and dilution of reservoir discharges to the river and therefore underestimate water quality impacts. Depending on the location of the discharge point relative to a channel bend or other channel junctions, the bathymetry of the channel and nature of the levees in the vicinity of the discharge point, cross-sectional mixing may or may not be complete. Field and multidimensional model-based data have demonstrated in some cases great cross-sectional variability of flows and water quality in Delta channels. I suggest the 3-dimensional aspects of flows and mixing in the vicinity of the proposed reservoir (and habitat) islands be explored with a multidimensional model and that such a modeling approach be used to refine placement of intakes and discharges. In addition, field investigations exploring cross-sectional variability would provide valuable data for use in conjunction with the 1D DSM2 and multidimensional models.

i) Early evening spikes in measured DO could be due to diel photosynthetic production [WQ 153], as acknowledged by the author [WQ 160]. Again, were these surface, bottom, mid-water column, or water column averaged measurements? Are we comparing a depth-averaged modeling approach to top- or bottom-biased measurements?

j) No references were provided for the DO calculation approach [WQ 154-156], except for the reaeration portion. Therefore, since I am not greatly experienced with DO modeling, I am not able to vouch for the reasonableness of the approach used. However, it appears generally reasonable.

k) If I understand it correctly, the "verification" of the approach discussed on WQ 158-159 should have been termed "calibration" since computed DO concentrations for Webb were adjusted to match Clifton Court intake and Antioch measurements. To do this, the algal growth rate was adjusted or "tuned." Therefore, I was not surprised when predicted DO at Webb "compared favorably" to measurements. My understanding is that a similar approach was taken for Bacon. If these simulations were in fact "tuned" to match observations, I do not think we can rely on their results as independently calibrated predictors of future responses. Essentially, calibration runs should not be considered the final, reliable output of a model---they are one or two steps previous to the final predictive output.

Further, although I appreciate the scarcity of available DO measurements, I am not comfortable with using potentially very different environments (though the most geographically proximate) for model calibration. What evidence is there that the calibration sites used are similar to the proposed DWP sites?

l) The DO "sag term" was based on SMARTS experiments, assuming the "tank experiments incorporated all the oxygen demand in the water column and sediments with the exception of algae respiration" [WQ 157]. What are all the relevant processes? What about processes having to do with respiration by vascular plants and animals? Although I am not an expert on DO dynamics and all the relevant terms, I am uncomfortable with the assumption that the SMARTS tanks incorporated (almost) all of the critical loss processes. I suggest this approach be reviewed by a DO expert.

m) I agree with the author's recommendations for additional studies [WQ 166], with the addition of exploration of processes beyond algae dynamics (e.g. macrophytes, epiphytes, benthic microalgae, etc.). I also echo my previous recommendation of using finer timesteps for resolving diel processes and using some multidimensional modeling approach to resolve vertical and horizontal variability.

Review #6

Review of:

CALFED/DWR In-Delta Storage Program Draft Reports

General Comments

Mercury

The discussion and analysis of potential mercury and methyl mercury problems in the proposed reservoir system are lacking in detail. Methyl mercury production is enhanced by an adequate supply of organically-bound mercury, very warm water temperatures, anaerobic conditions, high organic matter contents, dark waters that can block UV demethylation, and enhanced sulfate concentrations. All of these conditions appear to be present in the proposed storage reservoirs, suggesting that relatively high methyl mercury concentrations may be produced and released into the Delta channel system.

The methyl mercury formed in the reservoirs will probably be taken up by plankton which should form in fairly large numbers due to the ideal water temperature and high nutrient concentrations. The plankton should accumulate most of the methyl mercury, which will either be passed up the food chain to small fish inhabiting the reservoirs or into small fish once the reservoir is drained. Further bioaccumulation will follow.

Surprisingly, there were no findings or conclusions regarding potential mercury and methyl mercury problems in Chapter 6, Environmental Evaluations in the Draft Summary Report.

DOC

The release of DOC from submerged peat to the diverted waters was modeled using the logistic equation. This equation has three parameters of interest:

- the "A" term, which is the "maximum attainable DOC concentration";
- the "B" term, which relates to the initial concentration of DOC in the water'; and
- the "k" term, which is related to the rate of increase towards the maximum attainable DOC concentration.

The basic (although unstated) assumptions in the operation and design of the model as applied were:

- the reservoir is empty until mid winter;
- reservoir filling is accomplished during the winter months;
- the DOC model starts in mid-winter; and

- the reservoir is completely emptied before the following winter.

While the logistic model did approximate the shape of the DOC "growth" curves produced in the tank experiments, it is a poor choice for use in this situation for the following reasons.

1. It assumes a maximum DOC value exists which cannot be exceeded by the model; indeed, if the initial DOC concentration is higher than the "maximum DOC concentration", the DOC concentration of the water will decrease until it reaches the "maximum attainable DOC concentration", a peculiar situation encountered in the modeling exercises (p. 31 of the Water Quality Modeling Technical Appendix).

2. The increase in DOC concentrations observed in the tank experiments clearly show the influence of temperature on the rate of microbial decomposition of the underlying peat and the concurrent production of DOC. The reason the observed tank data show an asymptotic decline in DOC production rate towards the end of the cycle is because the data cycle starts in January and ends the following January and the temperatures towards the end of that 360 day period have also decreased sufficiently to greatly slow the microbial metabolic rate. If the data had been analyzed using a different starting date in a year long cycle (say July to July), the curve would have looked vastly different and a logistic model would have produced a very poor fit to the data and probably would not have been considered. Likewise, simulations using the logistic model with a starting date substantially different from January will produce a relatively poor prediction of the DOC behavior.

3. Due to the dominance of the "A" term, the logistic equation does not predict any increase in DOC concentration beyond the first year of reservoir filling. However, tank experiments showed additional increases in DOC concentrations in the second year of operation. This characteristic of the logistic model is particularly troubling since the simulation shows that the reservoirs would sometimes remain filled for more than one year (82-84 data).

4. What happens if the reservoir water quality characteristics are in exceedance of DOC (or other) criteria and the water cannot be released during the low flow period? The studies provided show that DOC concentrations in the reservoir waters will continue to increase if the water and/or peat temperatures are in a biologically active zone, so it seems likely that the problem would be exacerbated in the second and following years and continue to persist. One of the modeled years (82-84) shows one summer where the waters were not released; hence the waters would have been resident in the ponds for about 18 months.

5. A more accurate model would be to predict the flux of DOC from the peat to the water surface as a function of temperature. This rate is most likely a first order rate with respect to temperature and could either be modeled as such or could be modeled using a look up table. This would then allow time zero to be placed at any period of the year plus it would track DOC concentrations through multiple years.

6. The concept of a "maximum attainable DOC concentration" as expressed here is erroneous. There are no thermodynamic solubility constraints or other physical meanings to this term; in fact, it is a value estimated from a set of tank experiments. While the term may be useful from a modeling point of view, there is no scientific basis for the maximum concentration given. The form of the equation given presumes that you cannot exceed the "maximum DOC concentration", which is erroneous, and was illustrated in the data from which this equation was derived.

Although I don't think it's a very accurate model, the application of the logistic equation could be vastly improved by a simple modification. In the current model:

$$\mathbf{f(t) = A / (1 + B\ e^{-kt})} \qquad \text{[Eq. 1]}$$

where the "B" term is related to the DOC concentration at the initial time step, t(0). The model then proceeds to calculate an increase from this point to the "maximum attainable DOC concentration". In essence, the "B" term is a "head start" towards the "maximum attainable DOC concentration". If the initial DOC concentration is relatively high, then the rate of increase and the total increase observed are both very low. If the initial DOC concentration is greater than the "maximum attainable DOC concentration", the DOC concentration declines until it reaches the "maximum". However, we know from the SMARTS tank experiments that the DOC concentration can increase well beyond the "maximum".

I propose that the model be rearranged to the following:

$$\mathbf{f(t) = A / (1 + B^*\ e^{-kt}) + DOC_0} \qquad \text{[Eq. 2]}$$

where

B* represents a DOC concentration of 0.0 mg / L; and

DOC_0 is the DOC concentration of the waters at the initiation of the model, t=0.

This modification would allow for increases in DOC concentration well above the currently predicted "maximum attainable DOC concentration" providing the starting concentrations were high. Then the "A" term is no longer the "maximum attainable DOC concentration" but rather the maximum increase that can occur in a one year period, a much more meaningful term.

Interestingly, this is more or less how the model approach is initially described on page 64 of the *Synthesis of Data for Development of Reservoir Island Organic Carbon Model in DSM2 Model*, where it states:

"A simplified equation that incorporates the logistic equation, dilution factors, and diverted river DOC concentrations can be expressed as:

DOC(t) = DOC(0) + F(t)/Df [Eq. 3]

where DOC(0) is the diverted river DOC …"

An example is provided in this same draft report in the paragraph previous to this one which shows the effect of adding the DOC(0) term to the results of the logistic equation, producing a result higher than the "A" term. However, the model is then constructed as described earlier on p. 61, in the form given above in Eq. 1, thus eliminating the possibility of DOC concentrations exceeding the "A" term value.

It is unclear from the description what the initial DOC concentrations of the water used in the SMARTS tank experiments were. However, some of the graphs show values fairly close to zero, which suggests that the waters had relatively low DOC concentrations. If that's the case, then the "A" term was developed assuming DOC(0) had a concentration of essentially zero, and then the form of the equation given in Eq. 2 and 3 is more appropriate. Use of Eq. 3 would increase the predicted DOC concentration of the reservoir waters in the modeling effort by DOC(0). Consequently, a significantly higher percentage of the DOC concentrations would have been in excess of compliance criteria. Additional increases due to seepage water return and primary production within the reservoir system (which could be quite high) would also add to the modeled concentrations and produce even more non-compliant DOC values.

Since the model, as used, does not account for the initial DOC concentrations of the starting water (other than to use it as a "head start" towards the presumed "maximum attainable DOC concentrations"), is not adjusted to account for lower dilution values when the reservoirs are only partially filled, and all of the reservoir storage periods are similar in length, the modeled DOC concentrations at the time of release are all approximately equal as shown on the graph in Figs. 2.7a,band 2.8 a,b on pages 29 and 30 in the *Draft Report on Water Quality Investigations*. This is probably not the case in the real world, particularly if the waters are resident in the reservoir for more than one season.

DOC Criteria

Some of the criteria seem to change from one section of the report to another. For example, on p. 20 of the Draft Report on Water Quality Investigations, the criteria for TOC is: "The Project cannot cause an increase in TOC of more than 1.0 mg/L and it cannot cause TOC to exceed 4.0 mg/L at urban intakes. Yet on p. 35 of the Water Quality Modeling Technical Appendix the WQMP limit is stated as: "When the base case DOC is either less tan 3 mg/L or greater than 4.0 mg/L, the maximum increase in DOC is 1 mg/L. When the base case DOC is between 3 mg/L and 4 mg/L, then the alternative DOC can not exceed 4 mg/L …" These two criteria give different interpretations of behathe criteria when the DOC concentrations exceed 4 mg/L. Which is correct?

Dissolved Oxygen

The dissolved oxygen (DO) criterion for release of reservoir waters is if "the water discharged has a dissolved oxygen level of less than 6.0 mg/L or would depress the dissolved oxygen level in the adjacent channel of the Delta to less than 5.0 mg/L, or would depress the dissolved oxygen level in the reach of the San Joaquin River between Turner Cut and Stockton to less than 6.0 mg/L during September through November."

The wording of this criterion is rather odd. If the DO of the reservoir waters is greater than 6.0 mg/L, it seems unlikely that mixing it with the waters of the channel will depress the combined DO to less than 5.0 mg/L unless you can predict algal die-offs during mixing.

The modeled results show that this criterion is violated in algae free waters in nearly all of the summer period for the low organic carbon substrate and virtually all year for the high organic substrate. Low algae waters were predicted to violate the DO criterion during later summer and fall for the high organic carbon substrate. Unfortunately, the high organic carbon substrate also had deeper waters, so it was more difficult to separate out the individual effects of water depth from carbon content of the substrate.

The deeper waters present in the reservoir vs. the SMARTS tank experiments (20 ft. vs. 7 ft. depth) may push the DO concentrations lower. The higher organic carbon content and high light absorption characteristics of the overlying waters may restrict plankton growth at depth, further limiting oxygen production.

Specific Comments

In the survey of the islands for hazardous materials, were any mercurial or arsenical pesticides identified in the pesticide containers that were found? These two classes of pesticides have adsorptive, breakdown, persistence, and toxicity characteristics considerably different from organo-chlorine or organo-phosphate pesticides, and present substantially different concerns for water and environmental quality.

Draft Report on Water Quality Investigations

There is a large mismatch between the data shown in Figures 2-7a,b (p. 29) and the data in the tables used to (presumably) calculate some of these trends, Table 2B-6 (p.60), for example.

Table 2B-6 shows a monthly average release of 1844 cfs for July 80 whereas Fig 2-7a shows releases in May, June, and July, with no monthly average releases in excess of 750 cfs.

Similarly, Table 2B-6 shows a monthly average release of 1836 cfs in July 82 whereas the graph shows no, or very small releases, during that same period.

Numerous other dates do not appear to coincide between these two data sets.

The graphs also show increases in DOC during periods when the reservoir is either completely, or nearly completely, drained. In the model derivation, it is clear that the "A" term is derived from the final DOC concentrations in the tank experiments multiplied times a dilution factor related to water depth. If the reservoirs are not completely filled, then the "A" term should be adjusted accordingly to produce a similar rate of flux of DOC from the peat to the water. The rate of increase of DOC in the shallower water bodies, and the "maximum attainable DOC concentrations" shold be adjusted accordingly. I'm not sure if these periods of data affect the model badly, but they do appear a little odd.

While I can understand why relatively large Diversions of relatively low DOC water onto an island can cause decreases in the DOC concentrations of waters on the island, I fail to see why partial Releases of water from the island will cause a decrease in the DOC concentration of the waters remaining on the island. I believe this is an artifact of the model assumptions as currently incorporated. This behavior is graphically represented at the following dates in Fig 2-7a:

July 77
July 78
July 79
July 81
July 83
July 85

I also fail to see why partial Diversions of water onto the island cause large decreases in DOC concentration when the quantity of water added is insufficient to fully dilute the remaining water to such low levels. In the application of the logistic model to the calculation of DOC values in the reservoirs, it appears that the DOC concentrations in the reservoir waters at the time of diversion of water into the reservoirs might be ignored. In the logistic model, the "B" parameter is defined as the "starting DOC concentration", which appears to be the concentration in the reservoir water at the start of the time step. It appears in some of these calculations (Fig 2-7a,b) that the "B" parameter is calculated from the DOC concentration of the channel water and not the mixed channel/reservoir water that would actually constitute the starting materials. This is particularly troubling when the reservoir is partially filled at the time of diversion. However, in light of the numeric discrepancies between the graphs and the data purportedly used to construct them, compounded by the lack of diversion rate data, it is difficult to perform simple mass balance checks to determine the quantity of water still left in the reservoir or to assess the accuracy of the values displayed.

p. 25 - the text states that 8 major releases were made during the 16 year simulation period. This suggests that releases are made, on average, only once every two years. If that's the case, it becomes more critical that the DOC model be modified to account for concentration increases in subsequent years, and that whatever model is used should not include an "A" parameter which purports to represent a maximum DOC concentration. This alone will greatly improve the modeled output.

Water Quality Modeling Technical Appendix

It's interesting to note that inputs to the islands sometimes exceeded the "A" value in the DOC model and that the model then proceeded to "grow" the DOC concentrations by decreasing them until they met the "maximum DOC concentration".

To my understanding, the modeled increases in DOC only account for releases from the peat and do not include any factors for in situ production or DOC contributions from the return flow pumping, which might add an additional 1 mg/L to the concentrations. Additional increases due to in-reservoir productivity may swamp the increase resulting from releases from peat, which would bring much of the modeling into question. The model is not very conservative.

p. 35 - why does the WQMP allow increases of 1.0 mg/L when channel DOC is > 4.0, but less than that when it's between 3.0 and 4.0? Doesn't make much sense.

Releases of DOC exceeded the WQMP in 6 of the 8 release dates, which means 75% of the times that we are actually concerned with. The tables state that this would have only been a problem in a few percent of the months in the 13 year period. While that may be true, those happen to be the critical months of release. This type of data reporting is misleading and should be avoided.

Synthesis of Data for Development of Reservoir Island Organic Carbon Model in DSM2 Model

p. 10, 11, 13, 15, These are highly overfit models. The equation order is far too high, producing curves that do little to model the actual behavior of the data. A second order model ($y=a + bx + cx^2$) is probably sufficient. These data show what appears to be a 5th order model whereas the data have only 6 independent time periods. The curves so produced are meaningless, though they do fit well.

p. 52, last paragraph. "A standard agricultural saturated paste method was used." This method is unreferenced, but should be for the sake of the reviewers.

p. 52, last paragraph, "The holding period test conditions (e.g., redox potential, temperature) were not described." These data are very important parameters for interpretation of these results and should have been measured.

p. 69, 2nd paragraph. The figures used for the DOC concentration of seepage water have no real reference as to where they came from. I would presume that seepage waters would have DOC concentrations similar to that observed in the peat pore waters, which is around 200 mg / L, rather than the 20 mg / L they used. This would then add approximately 10 mg / L to the final DOC concentration in the reservoirs, not 1 mg / L as discussed here. However, there is no indication tha this 1 mg / L was incorporated into the estimates for the DOC concentration of the reservoir waters in the modeling efforts.

Draft Report on Environmental Evaluations

p. 55, 4th paragraph. The detection limits reported for mercury in surface and subsurface soils are "10 µg/L or ppb". Soils are not measured on a volume basis, and particularly not on a liquid measurement basis like a liter. In addition, the detection limit reported is excessively high, indicating the use of antiquated analytical methods. Modern detection limits for soils should easily be less than 0.05 ng/g, with overall detection limits of approximately 0.1 pg for the instrument. More accurate mercury profiles exist for soils from the Delta islands.

p. 55, 5th paragraph. Hg(0) is seldom deposited to aquatic systems as its solubility in water is exceedingly low. Typically, Hg(0) is released from waters by evasion or degassing processes. Likewise, Hg(0) is seldom converted to Hg(II) in waters; typically, the reverse reaction is more common. Much better sources for mercury behavior in water exist than Wetzel (2001).

Review #7

Review of CALFED In-Delta Storage Program's Reports on the Delta Wetlands Project

My review focuses on the hydrodynamic and water quality assessments of the proposed Delta Wetlands Projects. I have structured my comments according to the specific questions that were posed. In general, one of the most frustrating aspects of these reports is that great wealth of models and approaches that are used and the lack of coordination or iteration among them. It seems as if we are examining this animal by individual anatomical parts, rather than as a whole functioning organism. That maybe because we only have vague sketches of some of the vital parts. I hope that level of coordination comes at the next stage, as many of the individual assessments included here have merit and provide a basis for further investigation evaluation.

Models Used and their Assumptions

For the most part, these reports utilize existing hydrology and hydrodynamic models, albeit with some recent modifications e.g., to account for 'tides' driven by climatological as well as meteorological conditions. Indeed, as these models, such as CALSIM II, appear to be those currently used to assess operations and water management within the Delta they allow the project to be considered in the context of current delta operations. Thus, they provide an operational context for the water supply aspects of the project that may not be possible with hydrodynamic models that do not incorporate operational features of delta hydrodynamics.

However, it seems that most of the modeling conducted to assess the effects of reservoir filling and discharge from an operations and water quality standpoint use CALSIM output to drive DSM2. There is a need for iterative use of these two models in order that 'benefits' gained from reservoir discharge can be incorporated into system-wide water management activities and allowing assessment of the system-scale real-time operation of the DW project. Similarly, there are other acknowledged limitations in how the multiple models fit together for this evaluation. The DICU model, used to project consumptive uses in the Delta for 2020 level of development, does not incorporate any change in consumptive use associated with the project. Conversion of almost 20,000 acres of the delta to habitat and reservoir likely has some impact on consumptive use but the DICU model used here maintains the without project levels of use but distributes the use across the non-project parts of the Delta. This assumption is clearly stated in the reports, but it is not clear what impact it might have on the model results.

The water quality models that are used to assess the DOC derived from flooding of peat soils rely heavily on an experimental tank study. The study is described quite briefly and it s not clear how the tanks (experimental treatments) were replicated (the results are discussed by 'tank' implying that there was no replication) or the clear rationale for the experimental design. While the results of the experiment are used to provide a range – the high and low 'bookend' values – for DOC rather than a single value, it is impossible to tell from the level of detail provided how sensitive the values are to water depth, the nature of the soil, and the depth of the soil used in the experiment. Indeed, there is some discussion regarding the nature of the soil at Twitchell Island (the source of the experimental soils) being different from those at Webb and Bacon Tracts. While the use of an experiment to provide 'bounds' for a modeling effort in the absence of field data is appropriate, a greater acknowledgement of the factors the experiment fails to encompass is also necessary. This could be achieved through a well defined conceptual model of the processes involved in DOC 'release' from peat soils that identified which processes were examined in the experiment and those that were not.

The modeling of DOC which was reported, based on the peat soil issue, assumes that the islands are filled and then emptied with no further refilling. This seems to contradict the operational schedule which allows one of the islands to be refilled after initial discharge. This scenario may be too sophisticated for the DOC modeling to incorporate, which is understandable, but the difference between the simulated conditions and the proposed operational scenario should be acknowledged.

The DO and temperature models are spreadsheet models, but the equations used in the models are openly described and their assumptions are clear. However, there is little description of alternative models that may have been used, and what benefit they may have provided to the study, if more time had been available. The DO model discussion clearly acknowledges the limitation of using a mean daily evaluation of DO given the likely diurnal changes with DO decreasing at night. Both DOC and DO-temperature models assume the reservoirs will be well mixed based on wind conditions in the central delta. There is little consideration of the implications of this assumption even in general terms (e.g., were the reservoirs to stratify would this increase or decrease DOC concentration or discharge?)

In general the hydrologic and water quality modeling that is conducted is based on assessing the water supply benefits or the ability of the project to meet the SWRCB Decision 1643 criteria for operation. There appears to be no use of these models to optimize operation of the reservoirs to work within the constraints and maximize water supply benefits. It is possible that the lack of iteration among the models makes such scenario evaluation inappropriate at this stage, but it would seem that if a model can evaluate a constant discharge of 4000cfs then it could evaluate a varying discharge. It is possible that the commonly acknowledged drawback of evaluating flows on a mean daily basis makes this use of the current models unreasonable, but this use of models should be explored in the future.

Incorporation of important hydrological, biological and geochemical processes?
Climate change is an important factor for CALFED and other water resource agencies to consider in their planning efforts. There is some attempt to incorporate climate change into the operational evaluation of the project. However, it is not clear which climate change projections were used, and how they were selected. The statement 'DWR Flood Management Division made an assessment of flow variations as a result of climate change' is all the detail that is provided. It is not clear that changes in air temperature are included in the temperature and DO modeling, even though it is acknowledged that they are sensitive to temperature - both seasonal and daily fluctuations.

While the discussion of DOC and DO describe the important processes and acknowledge the role of algal/wetland plant productivity, important biological and biogeochemical processes are not considered in the models. There is a good discussion of the possible issues affecting phytoplankton growth and submerged aquatic vegetation, and the (limited) data available to constrain values for such productivity in these systems. There is also a sound conceptual model of the potential fates of macrophyte detritus. The inability of the current quantitative assessments to embrace this issue is a major shortcoming.

The assumption of all the reservoir water quality models in that the systems will be well mixed. While this may be correct due to high wind stress, it almost appears to be an assumption of convenience given that no data is available to verify models of stratified delta islands. The Environmental appendix includes some discussion to the effect that in late spring, before winds increase in the delta, there is potential for stratification and the isolation of relatively cold lower waters that may be preferentially discharged by pumps drawing from the lower reservoir. Stratification likely also decreases DO in the lower waters, again perhaps allowing the discharge of lower DO waters. However, there may be feedbacks to macrophyte growth etc that could influence the net quality of the discharges waters (as well as implications for mercury methylation in anoxic sediments). This possibility must be further explored.

Another set of processes which are not adequately encompassed in the reports are those associated with reservoir drawdown and drying. One of the limitations of the hydrodynamic models used in this study is that they cannot simulate wetting and drying of reservoir beds. Thus the reservoir is drawn down to a minimum 0.5 ft in some of the models, while operational schemes are unclear about the saturated/flooded condition of the soil post-discharge and pre-refill. The assessment of macrophyte growth assumes that water levels will be low enough to allow submerged aquatics to flourish, but drying may discourage them. The irregular topography of the islands likely means that some areas will be dry while others will be shallow flooded areas. The implications of shallow water, saturated soils, and unsaturated soils for soil and vegetative processes must be more explicitly considered.

Critical Gaps in Knowledge

Apart from the areas addressed above, there are two issues raised in the reports which appear to require more detailed information gathering before they can more fully considered as part of the project evaluation: seepage returns and the habitat value of the external levees. The seepage expected is substantial enough to require a detailed interception plan. The influence of seepage returns on water quality within the reservoirs is considered but in largely categorical terms (a mean value is assumed). A more detailed study of seepage from the islands and the magnitude of its effect on water quality must be conducted to better quantify this component of the DOC budget.

The reworking of the levee design apparently required for this project to be adopted by the State and Federal agencies will likely eradicate any habitat value of the external levees. Levees in this part of the delta frequently include tidal marshes, some shaded aquatic habitat and the riverine aquatic bed habitat. While the environmental assessment points to the importance of mid-channel island habitat in the vicinity of the project islands and questions whether its importance has been adequately considered, the habitat immediately adjacent to the levees should also be examined. Tidal wetlands, even of limited extent, which border delta distributary channels may provide higher quality habitat than those within flooded islands (such as Little Franks Tract). Their proximity to deepwater, flowing channels likely limits the growth of submerged aquatic vegetation and allows nekton direct access to refugia at the marsh edge. Also, exposed levee banks (sandy, subtidal, unvegetated) were found in BREACH studies to provide habitat for juvenile salmonids. Reworking of levee slopes to provide appropriate safety factors for reservoir levees will likely alter any such habitat by grading slopes and armoring. Any habitat losses need to be avoided or incorporated into the mitigation plan.

Further Research Needs

- Mercury. The effects of the reservoir and habitat islands on soil conditions and the possible release or methylation of mercury needs to be fully considered. The reports raise the questions about mercury but they must be addressed, and design/operation modified accordingly before this project goes forward.

- Fish screens. There are concerns regarding the design of the screens and these must be addressed. However, before the standard approved designs are incorporated by default a full evaluation of the potential effects, positive and negative, of screening these diversions (especially given the time of year when they will be operational - spring, cool water, native fishes) should be undertaken. The size and number of diversions should be examined relative to the efficacy of screening options.

- Models that can simulate temp stratification/DO in channel network. The concerns regarding temperature and DO in the discharge waters from the reservoirs are likely well founded. However, the models used to assess the fate of the discharged waters are insufficiently sophisticated to assess the real impact of the discharged waters on water quality in the delta. Models suggest that when water quality issues are more of a problem during times of flow reversal, and that discharges of DOC are drawn towards the Banks pumping plant as its exports increase to take advantage of the reservoir outflow. Both of these, and many other, scenarios indicate we need to assess the water quality dynamics of central and south delta channels preferably with 3-dimensional models that can account for bathymetric complexities and local stratification or mixing conditions that will affect the fate of reservoir outflows.

Post-Implementation Monitoring Needs

Mercury dynamics within the reservoir and habitat islands, and the fate of releases from the islands, must be understood to allow sound operation of the project. It seems likely that there are limited surrogates for a project like this and our modeling capabilities will be inadequate to fully project the effect on mercury prior to operation.

Real-time management or reservoir operations based on WQ criteria (both inside and outside of the islands). Because of the proximity of the project to the CVP and SWP as well as municipal intakes, it is essential that a real-time operations control system be developed to allow response to unpredicted events (such as a massive algal die-off prompting a dramatic drop in DO) or changing conditions allowing more flexibility re. 1643 criteria. This project should reflect our best and ever changing understanding of how to manage water. Thus real-time data must feed into a decision process allowing operations to be adapted and changed on a daily basis. Current regulatory constraints tend towards pre-set conditions but the relatively quick response available with this project should promote the development of a similarly responsive regulatory agreement that protects resources while maximizing the use of the freshwater that the project is designed to provide. Don’t build this and not be able to use it as efficiently as possible.

SECTION – E

In-Delta Storage Program
State Feasibility Study

Flow Science Inc. Reservoir Stratification Study June 2003 Reviews

SECTION E: RESERVOIR STRATIFICATION STUDY JUNE 2003 REVIEW

Review # 1

<u>Review of "In-Delta Reservoir Modeling" by Flow Science Inc., Prelim. Draft 2</u>

First, I compliment DWR for having contracted with Flow Science to investigate the potential for density stratification in the proposed reservoirs at Webb Tract and Bacon Island. Stratification, if present in the reservoirs, is a mechanism potentially impacting several aspects of the ecology and water quality in the reservoirs. Second, I believe Flow Science generally performed this investigation thoroughly, within the constraints of a one-dimensional vertical model and the information available on the proposed reservoir sites and their local environs. I believe a well-known and widely used 1D model like DYRESM was a logical tool for beginning to investigate vertical structure in the proposed reservoirs.

As the report states, wind, heat flux, convection, and in-/outflows all influence stratification development. In addition, total water depth affects the potential for stratification (if the water column is very shallow, then it is more easily mixed in the vertical, but if it is very deep, it is less easily completely mixed). Of all these factors, the report suggested that stratification is perhaps most sensitive to wind induced mixing. This makes sense to me; however, I think it may be useful to think of stratification as forming due to the confluence of a few "favorable" conditions: adequately warm air temperatures, adequately low wind, adequately deeply filled reservoirs, and minimal in-/outflows. Of course, what is "adequate" for one parameter depends on the other factors.

Three study years (1979, 1986, 1987) were chosen to represent a range of hydrologic/operational conditions. Then meteorological forcings from those three years were added to CALSIM-derived inflow and outflow results to drive the DYRESM simulations. Because no wind data were available precisely at the reservoir locations and to provide some range of wind stress for each simulation, wind data from different locations near the reservoir sites were used to provide a range encompassing what the reservoir sites probably would have experienced. Since these different data locations were shown to experience substantially different wind speeds, it is possible that the range used did encompass the overall range reservoir sites would experience *over time*. However, I cannot help but wonder how representative wind and other meteorological forcings from these three years are of the total range of conditions the reservoirs might experience over one or more decades. In other words, it was a valuable exercise to investigate wind speed variability in space, but how about interannual variability? Do meteorological forcings for 1979, 1986, and 1987 represent the full range and, if not, where do those wind, heating, etc. scenarios fall with respect to average and extreme conditions? Until we know that, we do not know for certain whether the full range of relevant meteorological conditions has been represented.

Another question I have is: Why were these 3 particular years chosen, when there was limited meteorological data available to drive the models?

Another detail I am curious about is the frequency of wind, radiation, etc. data used in the simulations, as well as the timestep of the simulations. I saw mention of a “daily record” but I don’t know whether that means daily mean values were used. If day-averaged values were used results could be different from a simulation using 10-min or hourly data. Also, the scalings relating wind data from various locations appear to be based on monthly means. Do the same scaling factors apply to the presumably higher frequency data used in the simulations? Further, the regressions (scaled & unscaled) of wind speed between Brentwood and Twitchell are not very significant ($R^2 = .3-.4$). Therefore, can we trust that a simple linear transformation provides a sound relationship between sites? Is there something more than just predictable shifts in magnitude that differentiate sites, such as timing? How much of an effect does this sort of error (i.e. the variability between sites not captured in the scaling) have on the results?

With respect to the river temperature data, it appears that for the 1979 simulation data for the SJR near Vernalis were used for both Webb and Bacon because there was “no significant difference” between those temperatures and measurements at Middle River near Bacon. Colleagues have recently examined temperature differences since 1970 for summer-fall across the Delta and found approx. 1.5 deg difference (or more) from the Webb region to the southern region containing Vernalis and southern Bacon. (Southern Bacon grouped well with Vernalis in terms of temperature.) Northern Bacon may differ from Vernalis by approx. 1 deg or more. For the 1986 & 1987 simulations, data from Antioch were used. Colleagues found that Antioch temperatures grouped well with the Webb region but may differ from the Bacon region by 0.5-1.5 degrees.These differences may have implications for predicted differences between reservoir waters and adjacent channel waters.

More details about which I am curious include:

What are the depths of the gates and pipes? Are these the depths used in the temperature and salinity comparisions between reservoirs and adjacent river channels?

Why is there compensating inflow and outflow during periods when the reservoir is full? (e.g. March-May 1986) Is the purpose of this to flush the reservoirs?

Why does salinity increase when no inflows are occurring? (evaporation?)

A model as widely used as DYRESM must have known error bounds for calculated variables like vertical temperature difference. How accurate is DYRESM usually? How are turbulent mixing coefficients calculated?

I am very interested in the mix of processes---and their relative threshold values---that control stratification. I think this study has been very useful in covering a range of probably realistic scenarios and in identifying critical mechanisms to which stratification appears especially sensitive (e.g. wind). However, it is not clear to me how much the development/prevention of stratification in the various scenarios is due to wind vs. heating vs. depth vs. inflow/outflow. In other words, how sensitive is stratification development to all the relevant processes? It is difficult to discern mechanisms when several parameters are varied at once. Is it possible to fix all but one parameter, systematically vary that one parameter across the full range of possible values, and then do the same for the other parameters? Such an exercise would move us beyond an intuitive feel for whether/how stratification develops to a very good understanding of the precise conditions that allow/prevent it. Such a solid understanding would allow us to quantitatively evaluate the percentage of time stratification should be expected and whether it should be expected for only extreme cases or on a regular basis. Further, a thorough understanding of the mechanisms and the relationships between them could ultimately allow for optimal design and operation of the reservoirs (e.g. manipulation of inflows/outflows).

I’d also like to comment that although these simulations suggest that prolonged stratification (for more than a few days) appears unlikely, the potential significance of short-lived stratification (e.g. 3-4 days) should not be discounted in terms of potential ecological and water quality impacts. To begin to assess the impact of such short-term stratification, time scales for other relevant processes (e.g. phytoplankton growth, DOC transformation, etc.) should be compared to the timescales for stratification and mixing to assess whether the stratification will likely make an impact.

Review #2

Review of “In-Delta Reservoir Modeling” by Flow Science Inc. (Draft 2; 9 June 2003)

Flow Science utilized an established one dimensional vertical mixing model driven by meteorology (DYRESM) to determine the potential for thermal stratification in reservoir islands (Webb Tract and Bacon Island) and to predict water temperatures in the island reservoirs versus the adjacent channels. Conditions representative of “typical”, dry and wet years were used for the simulations.

Overall, the report is clearly written and the analyses demonstrate competence with the use and limitations of DYRESM. In particular, recognition of the importance of the meteorological inputs is indicated by the careful comparison of data sets from nearby and more distant locations, and by the concluding recommendation for the installation of wind sensors at the actual sites. The key figures showing the seasonal variations in vertical stratification under different hydrological conditions will be valuable for further studies that link ecological and biogeochemical processes to the physical environment.

Clarification of a few points would improve the report:
1. In the model overview it is stated that horizontal variation in reservoirs is almost non-existent. Although this assumption underlies the proven utility of DYRESM, it is, in fact, incorrect. Experimental and field studies have amply demonstrated the existence and importance for transport and mixing of horizontal thermal gradients and of intrusions at various depths. DYRESM-WQ is not a “biochemical” model; it is, at best, an ecological/engineering model with many simplifying assumptions.

2. What was the averaging interval of the meteorological data?

3. Although DYRESM produces daily temperature profiles from daily input data, in turbid, warm, shallow systems, such as the island reservoirs, variations in meteorological conditions over a 24 hour period could produce repeated intervals of diel stratification, i.e., stratification for several hours each day. Depending on the rate of metabolic activity in the system, these intervals could result in altered biogeochemical conditions.

4. Although the modeling results can stand alone, there are relevant studies of similar systems that could augment the results presented.